Anne McLaren and the Human Embryo Research Debates in Britain, 1982–1990: Sociological Biography in the Analysis of Public Debate

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Declaration

This dissertation is the result of my own work and includes nothing that is the outcome of work done in collaboration, except as declared in the Preface or specified in the text. It is not substantially the same as any other work that I have submitted, or, is being concurrently submitted for a degree or diploma or other qualification at the University of Cambridge or any other university or similar institution, except as declared in the Preface or specified in the text. I further state that no substantial part of my dissertation has already been submitted, or is being concurrently submitted, for any such degree, diploma or other qualification at the University of Cambridge or any other university or similar institution, except as declared in the Preface or specified in the text. It does not exceed the prescribed word limit of 80,000 words.

Abstract

Anne McLaren and the Human Embryo Research Debates in Britain, 1982-1990: Sociological Biography in the Analysis of Public Debate, Marieke Louise Bigg

I describe the role of the scientist Dr Anne McLaren in the public and parliamentary debates on human fertilisation and embryology (HF&E) in Britain between 1982 and the resulting Act in 1990. A focus on McLaren's contributions to these debates elucidates how the case in favour of embryo research was constructed. Drawing especially on the use of the contested term 'pre-embryo' by the pro-human embryo research lobby to describe the subject of experimentation, and the development of pre-implantation genetic diagnosis (PGD) over the same time span as the debates, I show how a pro-research case was built around the idea that biology could be used towards broadly shared clinical goals. I tie this evolution of a case to McLaren's expertise, and show how McLaren combined an authoritative scientific understanding with the idea of moral purpose and social good, necessitating a language that could reflect both. I go on to draw on McLaren's biography, showing how she cultivated a 'style of practice' that relied on the non-literal translation of biological information to arrive at public consensus.

I engage with several areas of literature, beginning with the literature on the HF&E debates and analyses of the use of the term 'pre-embryo' in science and technology studies, and feminist cultural studies. I add a more detailed description of the specific biological arguments made by the pro-research lobby and show how these reflected a shifting position for the scientist in public debate. I also contribute to material-semiotic methodologies used in feminist cultural studies by scholars such as Donna Haraway, Evelyn Fox-Keller, and Susan Squier, showing how close readings of key documents can be used to extrapolate from an individual's life to better understand the causality of a public debate.

My combined archival research and interviews with McLaren's former collaborators leads me to foreground a particular tension around the legacy of the biological model used in the debates. I show how the scientists I interviewed repeatedly distanced themselves from the relativistic biological case they helped invent in the 1980s, describing their role instead as a literalist transmission of biological facts. An analysis of the work that McLaren did, I argue, serves as a reminder of the iterative process that scientists performed in order to arrive at a clinical translation. I use the metaphor of 'superimposition' to

explicate this process, a term used by McLaren to describe her own approach to policy-making, as well as in the writing of C.H. Waddington, who was a formative inspiration to her as a scientist. The metaphor helps to describe McLaren's method of layering social, legal, clinical, emotional and scientific arguments in order to build consensus based on overlapping concerns, and allows me to develop McLaren's role as a case study in broadening the conventional use of 'translation' in the post-millennial climate of biomedicine. I conclude that a multi-faceted understanding of the process of translation offers a productive and inclusive model for policy discussions, and one which continues to prescribe a role for scientists in the process of developing legislation in biomedicine today.

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Introduction

In 1978, Robert Edwards and Patrick Steptoe achieved a milestone in the history of reproductive biology: they had managed to fertilize an egg *in vitro* and place it in the womb of its mother, who then carried the child to term, leading to the birth of Louise Brown – the first 'IVF baby'. The advent of clinical IVF raised the possibility that research on human embryos might become part of clinical, as well as experimental, work on early human embryos and with this came the ethical implication that experiments would be needed to continue to improve the technique. The implications of this birth not only extended beyond their successful clinical application but marked a point of convergence between clinic and laboratory (Lee and Morgan 2001; Squier, 1994; Van Dyck, 1995). Indeed, the somewhat blurred distinction between clinical and experimental has been characteristic of IVF since 1978, as the IVF cycle as a clinical cycle continues to reveal basic scientific information, feeding into new research designs and applications, such as Pre-implantation Genetic Diagnosis (PGD), stem-cell science, and artificial gametes (Deech and Smajdor, 2007; Franklin, 2013b; Lee and Morgan, 2001).

The clinical translation of IVF made reproduction in general, but IVF in particular, a matter of public concern and the renegotiation of what was to be considered 'experimental' IVF became a point of public contestation (Mulkay, 1997; Yoxen, 1990). Various groups of the population demanded legal regulation, including feminists alarmed at the level of control these developments gave scientists over female bodies (see, for example: Burfoot, 1990; Crowe, 1990; Haimes, 1990; Price, 1990; Steinberg, 1990), as well as scientists as they confronted risks of being held liable for unpredicted or undesirable outcomes. In an attempt to address these concerns and to fill the legal vacuum generated by the emergence of IVF, the UK Government set up the Committee of Inquiry into Human Fertilisation and Embryology under the chairmanship of philosopher Mary Warnock in 1982. The Committee consisted of medics, social workers, lawyers and clerics, and was tasked with advising the Government, Parliament and the general public on the matter of human fertilisation and embryology. The Committee released its report in 1984, which was followed by six years of parliamentary and public debate. This lengthy period of contestation was characterised by governmental reluctance to take definitive action on the controversial issue of research, leading scientists to establish various bodies to provide information and reassurance to a generally uninformed and wary public (Crowe, 1990;

Deech and Smajdor, 2007; Franklin, 2013c; Gunning and English, 1993; Jasanoff, 2005; Lee and Morgan, 2001; Spallone, 1987; Steinberg, 1990; Strathern, 1992). This pro-research lobby was predominantly galvanised into action by the anti-research lobby's determination in pushing a Private Member's bill that, effectively, sought to ban research on human embryos in 1985 (Mulkay, 1997; Theodosiou and Johnson, 2012).

Scientists began to realise just how committed the opposition was to a campaign of action along lines employed previously in relation to the law on abortion, to which the only effective response was to form their own organised lobby (Mulkay, 1997, p. 42). In this process, the pro-lobby set up various organisations, including a Voluntary Licensing Authority (VLA), a joint initiative of the Medical Research Council and the Royal Society of Obstetricians and Gynaecologists in 1985, which began to issue the first licences for externally supervised research on IVF embryos. The VLA has been credited with providing the public with a sense that research could be regulated (Mulkay, 1997, p. 146), and, indeed, that this was already in effect. Another key organisation was PROGRESS, established in the same year as the VLA under the joint leadership of scientists, physicians and parliamentarians, who launched a campaign at the House of Commons intended to increase public understanding of, and support for, embryo research. This slowly shifted the weight of opinion in favour of research. In 1987, a White Paper was finally published, outlining proposed legislation, and presented to Parliament in 1989. The White Paper was then debated, amended, and eventually enacted in 1990.

The Committee's discussions began at a time when parliamentary and public opinion was especially hostile to basic scientific research on human embryos. In fact, as professors of law Morgan and Lee (2001) note in their text on human fertilisation and embryology, it is surprising that, considering the fervency of the opposition in 1984, Parliament came to approve research in the 1990 Act at all (p. 57). The period saw a conservative backlash to the permissive legislation of the latter half of the 1960s in 'matters of conscience', such as abortion (Chadwick and Wilson, 2018; Mulkay, 1997; Wilson, 2014). This meant that the debate was initially framed, like the abortion debates of the 1960s, as a question regarding the status of the entity that was considered to be the object of experimentation – the human embryo – and associated with the same pro-life arguments that placed the start of human life at conception, making experimentation, like abortion, equivalent to infanticide. In addition to this, the period was marked by an ambivalence towards science in general, both by left-wing scientists and intellectuals who reacted to the

injustices conducted by governments through science, as epitomised by the Vietnam War, and then by a Conservative government under Thatcher, which presented itself as sceptical of 'experts' (Chadwick and Wilson, 2018; Wilson, 2014). The Act proved, however, that the majority of parliamentarians had been swayed in favour of research that was previously considered to contravene the rights of the embryo as a potential human being.

In this thesis, I revisit this era of renegotiation of the relationship between lab and clinic, experimental and clinical, science and society, in the context of these debates. I do this from the perspective of the role played by a notable scientist who made a particularly consistent and foundational contribution to the arguments used by the pro-research lobby.

Dr Anne McLaren (1927–2007), renowned for her pioneering work in mammalian embryo development, both on the techniques that made IVF in humans possible and beyond, was the only embryologist on the Warnock Committee and was key in providing its members with a scientific understanding of the process of conception and its consequences for social and political organisation. McLaren has often been accredited by the community of policy-makers in Britain with providing a 'biological rationale' for one of the inquiry's key policy innovations, a cut-off point for human embryo experiments 14 days after fertilisation – the so-called '14-day rule'. The 14-day rule has been regarded as a key factor in securing public support for embryo research, and essential to what has been called the HF&E Act's 'social contract', in which the public allowed research on human embryos to continue in exchange for strict regulation (Franklin and Roberts, 2006).

McLaren's contributions to the committee, as Warnock has been all too eager to emphasise in her various talks on her process (e.g. The Anne McLaren Fund 10th Anniversary Conference, December, 2017; see also Warnock, 2001), extended well beyond providing scientific information. McLaren was keenly aware of the social questions at stake. From the moment she provided the Warnock Committee with her biological report, largely replicated under the same title in a 1984 published version *Where to Draw the Line?* McLaren became intimately involved in public discussions and displayed an awareness that the debate was not going to be based on scientific facts alone. The question of the permissibility of human embryo research, she believed, should instead be used to negotiate the relationship between science and society. The question of whether experimentation would be allowed to continue, after all, required a definition of what 'experimentation' was in light of the state of knowledge in the field, and this led McLaren to comment as much

on the prospects for clinical translation, as she did on her proper area of study – the basic science. This meant that, as much as people wanted to know that science would not go too far, they also needed to know what basic research meant to them, and what it could do for them. McLaren repeatedly made the point that, without legalising basic experiments to improve the technique, society would be condoning experimentation on women's bodies. Her biological case became part and parcel with this effort to define what was to be considered 'experimental' – a discussion that required science, ethics, law, and feelings. In this thesis, I describe Anne McLaren's role in the debates on human embryo research in the UK 1982–1990, in order to detail the process by which the pro-research lobby, driven by scientists, formed its case.

The biological model revisited

Despite Warnock's insistence on McLaren's indispensability to the Committee, McLaren's biological case for the 14-day rule also came to be associated with a less widely celebrated and, indeed, largely contested innovation – the introduction of a new biological distinction denoted by the term 'pre-embryo'. Despite broad critique of the arbitrariness of the term, some scholars have defended its efficacy, claiming that it allowed the pro-research lobby to begin to portray specimens younger than 14 days as an organism "ontologically distinguishable" from the embryo (Wilson, 2014, p. 164), and thus a suitable object of experimentation (Mulkay, 1997; Jasanoff, 2005). The British sociologist Michael Mulkay (1994; 1997), for example, claims, in his analysis of the mechanisms of the debate, that these arguments were key to the formation of a clear strategy by the pro-lobby, allowing them to give "coherent expression to the new image of the embryo" (Mulkay, 1994, p. 628) that undermined the "opponents' use of the phrase 'unborn children' while conveying to lay people that the potential subjects of laboratory experiment were not even proper human embryos" (Mulkay, 1997, p.31). Further arguments that were key to the pro-lobby victory included the possibility that research could reveal methods for controlling genetic disease, as well as a rhetoric of hope that promised the future reduction of suffering and an increase in people's access to normal family life (ibid). Despite the recognition of the term's role in the pro-research case in these accounts, it is thus considered to be one distinguishable aspect of the pro-lobby's case and of limited importance in delineating an ontological entity that would be a suitable research object (see e.g. Crowe, 1990; Jasanoff, 2005; Lee & Morgan, 2001; Mulkay, 1997; Spallone 1987). To the 'pre-embryo' these authors add several other factors, such as arguments about the prevention of genetic disease in Mulkay's

(1997) analysis.

In my interviews with scientists who participated in the pro-research lobby during the debates, there is a similar tendency to acknowledge the efficacy of the use of the term in the context of public deliberation, but to delineate it as one independent factor, often also as 'arbitrary', of limited importance, and secondary to the more decisive role of the arguments made for the benefits of genetic screening that would emerge from continued research. The term also, despite appearing repeatedly in the information material used by the pro-research lobby during the debates, was not used in the Warnock report itself, nor in the final legislation contained in the Human Fertilisation and Embryology Act in 1990. After introducing the term in public information pamphlet produced by the Voluntary Licensing Authority in 1985 (Minutes of the MRC and VLA, 26 March, RCOG Archives), it became the source of fervent critique in parliament and public press and produced open divisions in the scientific community throughout the debates that followed the release of the Warnock Report (see Davies, 1986; Chargaff, 1987; Kelly, 1990; Short, 1987 for critiques of the term, as well as my interviews with scientists in this thesis). Despite its utility, then, in convincing a wider public that research was acceptable, the 'pre-embryo' fell out of use in scientific publications and public discussions once the HF&E legislation had been secured. The 'pre-embryo', according to these histories and the interviews exploring its legacy included in this thesis, is repeatedly cast as part of a specific rhetorical strategy that marked a momentary and uncomfortable foray of a group of scientists into the unscientific domain of public debate; a historical artefact that is repeatedly, albeit confusedly, discredited.

In this dissertation, I look more closely into the precise construction and specific uses of the arguments around the 'pre-embryo' as they were developed by McLaren. I analyse her use of the term while IVF was still in the making, within the Warnock Committee through the six-year long debates that followed, and in retrospect. As well as reconstructing the history of the use of the term through close readings of various archival documents taken from Anne McLaren's archives at the British Library, the Royal College of Obstetricians and Gynaecologists and the National Archives at Kew, I talk to scientists about the legacy of the term today, and how they perceive and interpret its role in the debates of the 1980s. My analysis in **Chapter 1** shows that the term 'pre-embryo' was part of a strategy in which the biological case cannot be separated from those for genetic screening or the hopes presented by other future applications; that they all formed part of

the same iterative model of deliberation that McLaren developed over the course of the debates.

The efficacy of the 'pre-embryo' in delineating an ontological entity is only part of a broader effort to reframe the debates as centred around the reproduction of a society as a whole. I develop this analysis of the strategy used by the pro-research lobby in **Chapter 2**, where I show how the prospect of a clinical translation, pre-implantation genetic diagnosis (PGD), became an important reference point in the debates and the locus of a set of overlapping values, feelings and biological, legal and ethical arguments. The model as I describe it, can be used to deduce wide-ranging implications about the role of the scientist in public debate, the relationship between science, society, law and ethics, the public perception of biological facts and the role of science in reproduction, and the legacy of the debates in policy discussions about reproduction today.

Most importantly, an analysis of McLaren's framing of the biological account allows me to show exactly how the biological case in favour of human embryo research, now encoded in the HF&E Act, is entangled with the social, legal and ethical questions of how to govern human embryo research. I show how the biological facts are what Sarah Franklin (2013a) has called 'biologically relative' to a set of other domains of knowledge. This leads me to reflect on the relationships between the scientific model used in public debate and the role of clinical translation, and to develop the theme of 'translation', using McLaren's role in the debates as a case study in broadening the conventional use of the term in the post-millennial, 'translational' climate of biomedicine. McLaren, then, helps me recover the legacy of the debates, to frame their interpretation as a discussion of clinical translation, as opposed to one of the biological facts of development, or the status of the embryo, thereby recapitulating the process McLaren herself performed in the 1980s. I go on to describe more fully the approach to policy problem-solving evident in McLaren's contributions to the debates, by showing how the same approach – what I call her 'style of practice' – is evident across her scientific and political work in **Chapter 3**, developing the ways in which science factored into an ethical model of doing science, which, for McLaren, was deeply social. I thereby contribute to previous studies of the ways in which scientific authority was constituted during the debates that have foregrounded political and governmental machinations to explain legislative outcomes (e.g. Crowe, 1990; Jasanoff, 2005; Mulkay, 1997; van Dyck, 1995) by adding, in contrast, the account of one scientist's role. I hereby deepen an understanding of the process of policy-building in this era. In

Chapter 4, I go on to show how a multi-faceted understanding of the process of translation can offer a productive and inclusive ethical model for policy discussions that is as relevant to discussions about the 14-day rule today as it was in the 1980s.

In this dissertation, I look at McLaren in the 1980s and the intersectional and translational role she played between policy, parliament, government, ethics, law, science, and medicine. I draw on her 'style of practice', a term used by Science and Technology Studies scholar Evelyn Fox-Keller (1983), to describe the idiosyncratic approach taken by a scientist to making sense of their findings, how they engage with their research object, how they conduct, devise and interpret their experiments, and how they discuss that process. In this thesis, I characterise this style by reference to McLaren's expansive archive, as well as interviews with former collaborators, and trace how this is expressed not only in her science, but more broadly throughout her policy activities and social activism, in order to expose a dimension of the debates previously under-explored, namely the role of biological facts and of the biologist herself in the negotiations between science and society.

I demonstrate the principle argued by previous scholars on the debates, that biology during this era became relative to social questions (Franklin, 20013; Strathern, 1992), and reveal the implications of this shift by tying it to McLaren's role in the active shaping of the messages that were, or were not, received, in order to offer a developed analysis of the 1980s debates on embryology in Britain, as well as its legacy in the ongoing discussions about translation and regulation in biomedicine. My methodology thereby positions me in line with previous feminist science studies and cultural studies scholars, who understand scientific authority as being constituted through discourses, and consequently search for new stories to enter the "contest for public knowledge" with science, by choosing which texts to read and how to read them (Haraway, 1991, p. 82). Escaping "the fantasy of 'the one true meaning'" (Haraway in Davis and Turpin, 2015, p. 257, cited in Squier, 2017, p. 3) in this case "shifts the scope of debate, revealing a new vision of possibilities and limits" (Haraway, 1991, p. 83). This process of what has alternatively been called 'creating a labyrinth of narratives' (Ormiston and Sassower 1989), 'webbing' (Haraway 1992), or 'mapping' (van Dyck, 1995), treats reading as "a creative act that translates seemingly incompatible discourses into a cohesive symbolic representation" (van Dyck, 1995, p.60). I use this method to bring to light a new model for policy-building around reproductive biomedicine that prioritises social and systemic questions.

Methodology: Explorations in sociological biography

It was not clear from the onset how I was going to write a sociological biography, nor even exactly what this meant. I knew that, given McLaren's polymathic involvement with science, policy, politics, socialism, and public engagement with science more generally, I wanted to use McLaren as an indexical case study to reveal the broader social changes shaping the outcome of the human embryo research debates in Britain between 1984 and 1990. I wanted to use McLaren's role in the debates over embryology in the 1980s to analyse how the debate was shaped, influenced and constructed. I would focus in particular on her contributions to the legislative innovation that is arguably her most explicit contribution to the Human Fertilisation and Embryology Act, the so-called 14-day rule that limited experimentation on human embryos outside the human body to 14 days after conception, but would also look at her 'style of practice', a term used by Evelyn Fox Keller in her biography of the geneticist Barbara McClintock (1983) that describes the idiosyncratic approach taken by a scientist to making sense of their findings – how they engage with their research object, how they conduct, device and interpret their experiments, and how they discuss that process. Given her involvement in establishing the field of experimental biology since the 1950s, the development of the techniques that made possible therapies using embryonic stem cells, the fertility treatments such as IVF and PGD, her contribution to the Warnock Inquiry and subsequent debates, and her plethora of social and political roles in the Communist Party, the advancement of women in science, and family planning in developing countries to name a few, I wanted to draw on McLaren's intersectional role between policy, parliament, government, ethics, law, science, and medicine in the 1980s to ask how I could characterise her style of practice as a scientist and contributor to public debate, *as well as* what this style of practice revealed about the wider context in which she was operating. I wanted to analyse her values, professional attitude, and position and, in turn, to see how a close assessment of how these were implemented could inform an understanding of the causal factors driving the debate and its eventual resolution.

Feminist cultural studies

In order to develop my method, I drew on precedents set largely by scholars in feminist cultural studies, and arguably inaugurated by Donna Haraway with her 1989 monograph, *Primate Visions*, and her method of material-semiotic readings of history. In this method,

Haraway uses the analysis of the linguistic devices, framings and allusions of cultural artefacts to situate these in a history of making material ideological western values, and thereby instantiated the idea in feminism that national political economy can be read in representations of biology. Susan Squier (1994), in her book, *Babies in Bottles*, in this tack, draws on biography to show how the development of scientific ideas in reproductive science happened through interactions between literature and science that emerged from a network of families who were socialists, authors, and scientists, thereby situating her readings of specific texts in a broader historical era through an understanding of the networks of meaning and practice that shapes them. Squier draws on the lives of individuals, and the texts they produced, to interpolate their broader historical, political, social, economic context. She shows that science cannot be understood outside the idiosyncratic practice of individuals and the acts of persuasion required to achieve scientific consensus by these scientists, acts that in turn link these practitioners to a web of discourses and dominant ideologies that enable and restrict their concepts, their techniques, and their role in the development of scientific ideas.

Similarly, I focus on McLaren's participation in the debates leading to the enactment of the 1990 Human Fertilisation and Embryology Act to offer an indexical view of the forces and levers that shape the evolution of scientific knowledge. I do this primarily through close readings of McLaren's texts. Following cultural studies critics like José van Dyck (1995), Donna Haraway (1989), and Susan Squier (1994; 2017), I analyse the rhetorical construction of her arguments, the framing and appeal to discourses in specific texts used in the debates, to put these analyses together as a portrait of an era. I see science as constructed through discourses, defined as the "complex of signs and practices which assigns differential membership to a social or professional group", encompassing "the 'signs' but also, the 'practices' involved in language use, and that are unrestricted to the realms in which they arise" (van Dyck, 1995, p. 19). Science, in this view, achieves its cultural status through a "series of efforts to persuade relevant social actors that one's manufactured knowledge is the route to a desired form of objective power" (ibid., p. 577). Texts include *Where to Draw the Line* (McLaren, 1984), based on the document presented to the Warnock Committee as they developed their recommendation, McLaren's written contributions to the Voluntary Licensing Authority and Ad Hoc group on Legislation on Human Pre-Embryo Research (1987–1990), her work on the Medical research Council Advisory Committee, as well as presentations and reports presented to a plethora of

committees and working groups in the wake of the Act, such as The Human Embryology and Fertilisation Authority, and her ethics lectures at the World Health Organisation (1989). In what follows, I describe the method I have outlined in more detail by reference to key literature.

Critique as participation

Haraway, in *Primate Visions* (1989), shows what her approach to the cultural analysis of scientific knowledge can reveal about the ideologies inherent to knowledge creation. At one level, this is a book about the history of primatology, and about how ideas of nature, life and biology were shaped by the Cold War and other industrial changes to do with communication and control of information. Haraway also wants to show, however, how this military-industrial complex is very evident in the way that biological facts, such as accounts of evolution, are understood. She shows this through specific analyses, such as the diorama at the American Museum of Natural History dedicated to Roosevelt in 1936, and in turn how this inscribes a certain vision of empire into the animal kingdom, how this vision comes to substantiate the body politic as part of a 'natural' order, and how this body politic becomes how people think about themselves (e.g. as non-animal and civilised as opposed to unmediated, primitive nature). The book thereby sets out a distinctive method. It is a cultural feminist analysis of science. The analysis of the linguistic devices, framings and allusions of cultural artefacts is used to situate these in a history of making material ideological western values, and thereby instantiated the idea in feminism that national political economy could be read in representations of biology. Haraway explains what this method means in terms of her understanding of the history of scientific knowledge:

> By history I mean a corrosive sense of the contradictions and multiple material-semiotic processes at the heart of scientific knowledge. History is not a completed past simply waiting to be applied to deepen a time probe or to give perspective. It is a discipline reworked by postmodern insights about always split, fragmented, and multiple subjects, identities, and collectivities. All units and actors cohere partially and provisionally, held together by complex material-semiotic-social practices. In the space opened up by such contradictions and multiplicities lies the possibility for reflexive responsibility for the shape of narrative fields.
>
> Haraway, 1989, p.172

The critic can now challenge scientific knowledge through active participation in the construction of meaning, rather than confrontation with a stable domain of science. By drawing on existing discourses and contributing new ones, feminists can renegotiate the

relationships that come to constitute meaning and practice in science. Moreover, as van Dyck (1995) explains, "the awareness that power is always coextensive with discursive structures funnels her attention to discourse and language" so that "feminist assessments of new reproductive technologies do not only lead to the investigation of gender constructions, but also of genre constructions: a reconsideration of conventional ways of knowing and representing the reproductive body" (p. 103). Feminism, in this sense, becomes "a search for new stories, and so for a language which names a new vision of possibilities and limits" (Haraway, 1991, p. 82). Feminist cultural studies, in turn, becomes about entering into the "contest for public knowledge" with science (ibid) by choosing what texts to read and how to read them. This is the process of the 'recontextualization' of discourses. Others have called this 'creating a labyrinth of narratives' (Ormiston and Sassower 1989), 'webbing' (Haraway 1992), or 'mapping' (van Dyck, 1995). In each of these conceptualisations, reading is "a creative act that translates seemingly incompatible discourses into a cohesive symbolic representation" (van Dyck, 1995, p.60).

I implement this idea of searching for new texts in order to map a debate. I read McLaren's scientific texts, speeches to scientific, political, and public audiences, and journal articles alike as instances of her negotiating scientific facts. In this way, I open up the discussion of what science is, of what it is allowed to entail. Moreover, I describe McLaren's style of practice to show how she herself saw science as much more encompassing than the assertions contained in the scientific article, or even the laboratory work that is done to substantiate them. Both my reading of McLaren and McLaren's practice itself comes to open up a view that demands faculties and new domains conventionally not associated with the narrow remit of the laboratory scientist, as engagement with public debate and policy discussions becomes central to their practice.

The individual and mapping debate

The project was, in part, also a methodological experiment in using individual biography, rather than paradigms, to understand an era and a public debate. Unlike the models of action developed to describe the emergence of scientific consensus in Actor Network Theory in science studies (e.g. Callon, 1986; Fujimura, 1987, 1988; Hacking, 1983; Latour and Woolgar, 1979; Pickering, 1995; Star and Griesemer, 1989; Law, 1990) that draw on networks of interconnected actors to describe how scientific ideas are adopted, I was going to depart from the description of an individual and attempt to deduce causal connections

from her own personal, intellectual, and political formation and the means she used to influence public debate. While in my analysis I acknowledge the existence of what Sheila Jasanoff calls 'cultural framings' that define the regulatory problems presented by new biotechnologies on a national level (2005), I also hold that, as Paul Rabinow shows in his ethnography of the invention of the polymerase chain reaction (PCR) (1996), individuals can reframe culture too. I ask how one such individual is linked to the social whole, how we can explain and understand an individual's role and how this features in a sociological analysis of public debate. Drawing on her role in the debates, as well as her involvement on other committees, as a Marxist, in the scientific community, against the background of her earlier years, and in light of her scientific legacy, I use McLaren's role in the debates to test previous key analyses of the causal drivers in the pro-lobby success in the debates on human embryo research, especially British sociologist Michael Mulkay's (1997) detailed analysis of the parliamentary record and news coverage. In turn, I use this analysis to draw conclusions about more macro-sociological phenomena, such as the role of scientific expertise in the UK debate and the specificity of the public debate in the UK. I now turn to a comparison between two biographies of the same scientist to show how the method of material-semiotic reading described above provides a particularly apt approach to biography, especially when drawing on the individual's role to map a debate.

Biography and science

In thinking through how I would use the account of an individual to reflect on a broader, public debate, the biography of the geneticist Barbara McClintock was a key resource. In 1983, the scientist-turned-historian of science, Evelyn Fox-Keller, wrote a biography that was to become a cornerstone in the feminist critique of science. In the biography, Keller tells the story of McClintock's career, especially her theory of gene transposition developed in the late 1940s and early 1950s, but which was not recognized by the scientific community until the 1970s. Keller draws on this case to reflect on the construction of consent within a scientific community. "A new idea", she writes, "is born in the privacy of one man's or one woman's dreams" but "for that conception to become part of the body of scientific theory, it must be acknowledged by society of which the individual is a member" (Keller, 1983, p. xx). Scientific knowledge thus depends on the interaction between "individual creativity and communal validation" (ibid). Keller offers an explanation, a causal account of a particular discovery in the history of science, and approaches this not by referring to existing paradigms, but through a detailed and textured account of an

individual's style. She explains,

> … the individual … the idiosyncrasies of autobiography and personality that incline an individual scientist to a particular set of methodological and philosophical commitments, to resisting or accepting the dominant trend within a field – *but always against the backdrop of community*. Of necessity, therefore, this book must serve simultaneously as biography and as intellectual history. It's starting point is the recognition that science is at once a highly personal and a communal endeavour.
>
> Keller, 1983, p. xxi

Keller implements this approach in a method that relies centrally on an extensive, immersive interview with McClintock herself, and draws on this to *describe* closely how McClintock practiced science. From this she deduces what the epistemological implications of this approach are and shows how this differed from the dominant models in the field of genetics at the time. Key to this approach, then, is a meticulous description of McClintock's so-called style: an "account of the individual's "style" as a scientist – a style partly learned and partly self-generated" (ibid., p. xxi). Keller does not, as she might have in a conventional historical account, explain McClintock's initial marginalisation in her field in terms of the criteria for knowledge of the discipline – or at least, not only. Keller uses her interview with McClintock partly to show where her style of 'doing science' resonated with dominant ideas in genetics at the time and where it did not, but she also importantly lays out McClintock's account of her own career, how she thought through scientific questions, and the obstacles she had to negotiate in order to understand her scientific problems. She adds to the history of science a detailed account from McClintock's vantage point to show exactly how her approach developed, that describes her divergence in terms of a style of practising science and that shows how her findings were not always 'heard' because she did not adhere to conventional modes of expression in science. Keller's interest in the criteria for knowledge in science thus leads her to a discussion of language, in what she comes to conceptualise as specific acts of 'persuasion'. The delay in the response to McClintock's theories was not only a result of its "revolutionary implications" in the historical context of genetics, nor even just her "particular nature of her knowledge and understanding" (ibid., p. 268), but also in concrete terms, in the specific language used to convince.

These acts of persuasion shape the interpretation of evidence, relate it to existing knowledge in a particular way, and determine the success or failure of a discovery. Also, importantly, communication relies on common understandings and assumed practices, or

conventions. When science is placed in this space of hierarchical and material acts of persuasion, science itself becomes a social practice, emphasising the critical importance of understanding an individual's idiosyncrasies in their attempts to disseminate their findings to their professional community. Keller's close reading of an interview set against a specific historical series of events thus offers not only an account of why McClintock's discovery of transposition was not immediately taken up, but also allows her to show *how* this negotiation plays out in practice and thus illuminates the causal levers that determine which scientific ideas rise to the fore. Keller uses McClintock to make a point not only about the role of 'non-scientific' elements in discovery, but also in persuasion:

> The importance of what are sometimes called extrarational or extralogical components of thought in the *discovery* of a new principle or law is generally acknowledged….But the role of these extralogical components in persuasion and acceptance (in making an argument convincing) is less frequently discussed, partly because they are less visible. The ways in which the credibility or effectiveness of an argument depends on a realm of common experiences, on extensive experience in communicating those experiences in a common language, are hard to see precisely because such commonalities are taken for granted. Only when we step outside of the "consensual domain" – when we stand on the periphery of a community with a common language – do we begin to become aware of the unarticulated premises, mutual understandings, and assumed practices of the group.
>
> Keller, 1983, pp. 145–146

McClintock provides a case in point: the ways in which McClintock was marginal to the consensual domain are multiple, extending beyond her use of the very particular language of cytogenetics, to an overarching style characterised by an openness about her process and practice in science. Yet Keller's interest is not only in how a discovery relates to dominant paradigms for understanding, but in how we can open up the process by which a discovery becomes accepted to inspection. This question demands a close, meticulous description and analysis of the words McClintock herself uses to describe her process. Not only does the story of Barbara McClintock allow us to "explore the conditions under which dissent in science arises", but a detailed description opens up a way of understanding the exact "function it serves, and the plurality of values and goals it reflects" (Keller, 1983, p. xx). "It makes us ask", Keller writes, "What role do interests, individual and collective, play in the evolution of scientific knowledge?" (ibid). Keller thus enacts her commitment to unearthing marginal or excluded languages by taking seriously the language of an individual with a set of ideas that challenged the conventional way of speaking about science.

Keller's biography, as well as McClintock's science, have both found recognition since the early 1980s. The American historian Nathanial Comfort (2001), in his alternative account of McClintock's life's work, describes the afterlife of Keller's biography. He claims that Keller and McClintock "fuelled each other's fame" and that Keller's book with its "timely" message became the foundation for a story that "seemed to epitomise women's experience in science" and was taken up as a case in point in the feminist critique of science (p. 6). He argues that Keller's biography was then taken up in a broader "manifesto of feminine science" (Keller, cited in Comfort, 2001, p. 6), which Keller herself bemoaned, on the grounds that she had hoped to open avenues for thinking about science that extended beyond binary, gendered norms. In this critical literature, Comfort claims, McClintock represented a different way of doing science, a 'feminine' approach, that focused on the individual in such a way as to combine the rational and emotional sides of intellect, in a way that was not masculine, "dominating, controlling, reductionist, rational, and linear" but "holistic, intuitive, interactionist, even mystical" (Comfort, p. 6).

It is worth at this point comparing Comfort's methodological approach to Keller's in order to explicate how Keller's approach reflects her research question, how this differs from Comfort's question, and how her project is liable to being misunderstood. For Comfort, the afterlife of the Keller's biography of McClintock outlined above is cause for an intervention. The fact that the biography was taken up by feminists to assert a different way of doing science appears unnecessarily essentialist to Comfort, perpetuating gendered models of doing science, and not an accurate reflection of McClintock's contributions to the field of genetics. His critique, however, seems to skirt over the nuance of Keller's original biography, focusing more on the book's reception than the book itself. His critique of Keller seems to be that her book has been politicised, which he suggests is unscholarly, and that this propensity to politicisation is already reflected in certain biases in Keller's methodology. Comfort explains how, unlike Keller who focused on a single interview, he draws on "correspondence and research notes" as well as the McClintock's research papers, which are notoriously difficult to read, to give a story "much more complex" than "the one McClintock gave reporters and interviewers" (Comfort, p. 5). It seems that he sees in Keller's method an uncritical and blind acceptance of McClintock's account. To rectify this misinterpretation of the McClintock story, Comfort proposes a different methodological approach, in the form of 'history', as opposed to 'mythology':

In the course of my research, many people have told me that McClintock detested

> the status she attained as a feminist icon. Peeling back the mask reveals what is left out of the myth: the intimidating intellect, and fierce independence of McClintock the woman; her high visibility and continuing presence in the genetics community; and the complexity and historical situation of her science. In short, by examining *her not as representative of a class but as a unique individual, we get to know a new Barbara McClintock, one with a distinctive style and a misunderstood contribution to the history of biological thought.*
>
> Comfort, 2001, p. 8 [emphasis added]

As opposed to Keller's narrow focus, Comfort claims to add the perspective offered by comparison between various interviews and new archival material. This allows him, he contends, to reconstruct the experiments in order to place them in a broader context consisting of various scientific fields, and allows the implications of certain events, like the rediscovery of transposition in 1970s, to be more fully understood. He argues that this is needed to move beyond the politicised, misconstrued version of McClintock in feminist criticism. He, ironically, despite his critique of the essentialist model of feminine science made by feminists, makes a distinction between the figure as a public myth and their 'essential identity', what he calls the 'figure under the carpet', the woman behind the mask. While he and Keller seem to agree, then, on the importance of describing a scientist's style of thought, Comfort is interested in describing a singular, true version of McClintock and in using this to situate her in relation to an existing set of fields and paradigms in science; her attempts to explain her idiosyncrasy within the accepted language offered in the conventional modes of science. He says of what was described by Keller as McClintock's 'feeling for the organism':

> McClintock's allegedly holistic, intuitive scientific style was in fact highly rational and based on immense experience and reading. All good scientists develop a feel for their experimental material. McClintock was distinctive in the speed and facility with which she solved problems and in her emphasis on the synthesis that follows reduction in any complete solution to a scientific problem.
>
> Comfort, 2001, p. 9

Comfort counters the 'unscientific' language of affect used by Keller and others to describe McClintock's style, betraying his aim to make her understood within an accepted set of scientific ideas and tradition of thought. Keller's interest, however, lies in describing exactly how McClintock and her ideas came to relate to the scientific community, and to show what this tells us about the work of doing science itself, the causal dynamics by which it takes shape from the perspective of the individual. Keller is not interested in describing McClintock's idiosyncrasies in order to situate her, but in using an interpolation of her

specific utterances to reflect on the criteria for truth in science, in what she is *saying* about *doing* science, not about who she is or the place of her discoveries after the fact in a history of a discipline. For Keller, like Comfort, McClintock was not remarkable for her commitment to holism. She shared with many 'rational' scientists "the credo that nature is lawful, and the dedication to the task of articulating those laws" as well as "the additional awareness that reason and experiment, generally claimed to be principal means of this pursuit, do not suffice" (Keller, 1983, p. 200). Many scientists have thought this way, Keller writes, and the vision of a rational science has coexisted with an image of science expressed as a "deep reverence for nature, a capacity for union with that which is to be known" throughout history" (ibid). She continues,

> In all this, McClintock is no exception. What is exceptional is her forthrightness of expression – the pride she takes in holding, and voicing, attitudes that run counter to our more customary ideas about science. In her mind, what we call the scientific method cannot by itself give us "real understanding". "It gives us relationships which are useful, valid, and technically marvellous; however, they are not truth" (McClintock, 1978). And it is by no means the only way of acquiring knowledge.
>
> Keller, 1983, p. 201

An emphasis on McClintock's 'style', for Keller, is a way of focusing on her relation to the process of negotiating knowledge, a move away from essentialising notions of 'personality', but a description of a specific approach, deduced from contextualised utterances, where the individual is always read against a backdrop of 'community'. Keller's approach requires detailed close readings, taking the supposedly limited source-material offered by a single interview seriously, and taking McClintock's own language, metaphors, and paradigms seriously, in order to develop an understanding of how meaning is made through the interaction between individual and the multiple discourses in which they find themselves. For Keller, what is interesting about McClintock is her ability to articulate and describe in detail the aspects of scientific practice that are not expressed in the language of scientific journals or the rhetoric of scientists in general. Not only the story of the acceptance of her work, but her style as a whole, which encompasses the way she talks about science, her method, the way she engages with scientists, and the way she writes about it, make her a remarkable and rich case study for assessing how scientific knowledge is constructed.

McClintock represents not only an object of study, but her language itself provides a tool or a lens through which to think through the negotiation of knowledge. Comfort

extensively describes McClintock's style of thought as a set of ideas on the boundary of several disciplines and their respective criteria for knowledge. What a detailed description of a style of practice adds, is the ability to trace how ideas are expressed and formed, to locate the resistances and tensions that arise, and thereby the boundaries of paradigms for understanding in science as they take shape, in order to bring unacknowledged elements in the construction of knowledge to light.

In my thesis, then, Comfort's reading of Keller helps me describe the distinctiveness of Keller's approach as a sociology of science. While I can appreciate some of Comfort's concerns about the reception of Keller's work and her heavy reliance on a single interview, his reading of her biography is reductive in exactly the sense that he claims Keller's view of McClintock is, namely that he reads it only as a gendered argument. A more generous reading of Keller would show how she is constructing, through her focus on McClintock's style of practice, a biography that appeals to a set of reference points outside of the history of a discipline or character traits of the individual, in order to bring to light the causal dynamics of the constitution of a particular set of facts, and to tell a life in its deeply embedded social context. In doing so, Keller offers a model that I draw upon in this dissertation.

In developing my methodology, I thus take inspiration from Keller, also vis-à-vis my reading of Comfort, which, in itself, is a reading of Keller, which is a reading of McClintock's interview. This is just one of the ways in which my methodology relies on 'readings of readings of readings', a conceptual point that comes to bear on my analysis in important ways. My reading of Comfort helps foreground an important parallel between Keller's work on McClintock, and my reading of Anne McLaren's writing, namely the sense in which McLaren's style of practice serves as an object of study, a life that needs to be explained, as well as a tool through which to understand the process of knowledge production itself – *sociologically*. An example helps to show how this resonance has enriched my methodology.

In one of the most striking passages in Keller's biography of McClintock, McClintock uses the analogy of a computer to describe how she cognitively processes her experimental observations. The mind, she says, functions "like a computer" – processing and integrating data far more complex than we can be conscious of. As she observed the corn in her experiments, all she was conscious of doing was "looking at these fine stripes of recessive tissue"; she says the computer did the rest. "And I never made a mistake" (cited

in Keller, 1983, p. 102). She goes on,

> It is done with complete confidence, complete understanding. I understood every plant. Without being able to know what it was I was integrating, I *understood* the phenotype. … It means I was using a computer that was working very rapidly and perfectly. I couldn't train anyone to do that.
>
> McClintock in Keller, 1983, p. 103

McClintock describes a process of integrating her data by referring to what Keller calls an internalised "point of reference" (p. 126), based on the sum total of her experimental observations and thinking that serves as an "organising scheme" (ibid) for the data she needs to interpret and represents a type of understanding that surpasses what she is directly conscious of. Keller explicates this method of doing science to reflect on the nexus between affect and knowledge. This computer was "mediating between the spots, the patterns they formed, and her internal vision" (Keller, p. 126). The internal vision, which McClintock describes using the impersonal, 'unfeminine', technical language of computation, is based on an intimate, affective, 'feeling for the organism' developed through an intimate relationship over time, and results in a striking confidence and conviction in her scientific interpretation. This unconventional description of her scientific process, Keller argues, explains the unreceptive response to her findings in the scientific community – it was the language she used to describe her methodology that fell outside of the conventions of the field.

Comfort's critique of Keller's 'unscientific' use of limited source material betrays his misunderstanding of the role that McClintock's account plays in Keller's analysis. As we have seen, Keller wants to explain McClintock's role in the history of her field(s), but this is done through an analysis of McClintock's own language, in order to build upon these reference points as a basis for an account, exploring how these came to connect, or not, to dominant discourses at the time. It is in this sense that McClintock serves as both the object of study to be explained, and the tool through which a new account is built. Keller reads this methodological duality in McClintock's account of her own process. Here, too, the corn provides both the object of study, at the same time that it is translated into experimental data that is integrated into an evolving internal, conceptual model of the relationships she is explaining – it is about understanding the corn and understanding *through* the corn; it is thus this self-reflexive process of modelling that eventually explains how the inanimate computer of the mind provides the basis for a 'feeling for the organism', how both

mathematical model and affect are involved in the ongoing negotiation of the relation between scientist and object, or social scientist and interviewee.

Hermeneutics of practice

The centrality of the *process* of interpretation to Keller's reading of McClintock, and the way in which this process is recapitulated by Keller herself in her immersion in McClintock's own reconstructions through language, is an approach that informs my understanding of my relationship to McLaren's texts in this project. The approach I take is hermeneutic, by which I allude to the practice of textual interpretation in the humanities, derived from biblical study, in which deep readings of a text transcend the text in so far as they provide insight into the 'meta-textual' that, at the same time, is inherent to the nature of the word itself; the word always acquires its meaning within a historical, political, social context. Hermeneutics has been integral to poststructuralism in sociology and was deployed by members of the 'interpretative turn' in the 1970s, as a radicalising force for the understanding of social science. Anthropologists Paul Rabinow and Robert Sullivan (1979) wrote in *Interpretative Social Science* that they were drawing on the analogy between text and action, textual meanings and social meanings, to revitalise social science and move it away from an economistic, scientistic, rationalistic paradigm towards a humanities paradigm in which we can understand social meaning to be as impactful as social structure. Unlike the previous epistemological models in the sciences, such as logical empiricism, and even the later systems approaches, such as structuralism, within the human sciences – all based on the opposition between subjective and objective – the interpretive approach rejects the possibility of reducing the world of signification to the stable "products of self-consciousness" (p.5). Instead, they assert, interpretation must always begin from within **"**the web of meaning" that "constitutes human existence" (p. 5). As the philosopher Charles Taylor reiterates in his contribution to the anthology, "meaning is for a subject, in a situation; it is about something; and it exists as part of a field; there are no simple elements of meaning" and this, in turn, means that "human life … cannot be shielded from open interference and studied in a vacuum or a scientifically controlled environment", but instead our "capacity to understand is rooted in our own self-definitions", in a cultural world consisting of the "web of signification we ourselves have spun" (ibid). There is no outside or detached standpoint from which to observe. On the contrary, when we try to understand the cultural world, Taylor writes, "we are dealing with interpretations and interpretations of interpretations" (Taylor, cited in Rabinow and Sullivan, 1979, p. 6).

In trying to develop a methodology in which, given this understanding of meanings as interpretation, we might still mediate and judge between conflicting versions, scholars of the interpretive turn looked to a "transformed version of textual criticism in the humanities" (ibid). To understand a text is to follow its movements from "sense to reference", from what it says to what it talks about, and so the role of the critic becomes to "display the power of disclosure implied in" the author's "discourse beyond the limited horizon of his own existential situation" by exposing the conditions of its existence (Ricoeur, cited in Rabinow and Sullivan, 1979, p. 100). In this approach, "understanding any action is analogous to textual interpretation" (ibid).

This means that the intelligibility of any action requires reference to its larger context: a cultural world. The American anthropologist Clifford Geertz (1972), in this way, describes the Balinese cockfight as a cultural script, by progressively incorporating other essential national symbols, institutions, and practices that he shows are necessary to an understanding of this "event" (Geertz, p. 84). Again, the role of the critic is itself one of interpretation, that re-enacts the epistemology on which the method is based. The aim for Geertz is "not to uncover universals or laws but rather to explicate context and the world" (Rabinow and Sullivan on Geertz, 1958, p. 13) by using the cockfight not as an object to be understood, but as a node in a web of meaning to be contextualised. It is in this sense, like McClintock for Keller, or corn for McClintock, that the critic performs a process of understanding 'with' and 'through'.

In my reading of McLaren, I use a similar hermeneutic approach in order to explicate the 'meaning' of a life. I show how a particular style of practice was exercised across sectors, describing the particular cultural and social worlds through which this was enacted through close readings of texts, to draw together these meanings in my own interpretation of this life as indexical of a cultural-political moment in the legislation on biomedicine. I read McLaren, like Geertz's cockfight, as a cultural script, as a condensation of meaning that needs to be unravelled through readings of her utterances in their always deeply embedded context. I also use this hermeneutical reading of a style of practice as a way of reading history 'sociologically', by which I mean that I read the history of the public debate on human embryo research in Britain as a history of social transformation, a history of the birth of a law not just in terms of what the sociologist Michael Mulkay (1997) describes in his analysis of the debates as the outcome of political machinations, but through the actions of the people who had a transformative role. Their role was not just as

individuals in a situation, but as individuals over a lifetime by establishing a style of practice that was influential over and over again across different sectors in a way that was transformative. Meaning, in my account, only emerges in a context, as 'meaning for', and can only be summarised an accumulation of these instances.

I furthermore show that McLaren's role was transformative precisely because she also had an unconventional understanding of the role that scientists should play in public debate. She brought this transformative insight to social situations. In McLaren's view, as I show throughout this dissertation, scientific models were developed very much through what I call the 'translational' work of reconciling conflicting meanings in order to arrive at a way forward in the form of a model relationship between domains – biological, legal, clinical, ethical –in answer to a particular question. This solution makes sense pragmatically, but not, in her view, 'definitively' or 'objectively', as much as that word even applies in a discussion of an 'open-system' that can only ever be understood from the vantage point of the embedded individual. McLaren, then, exercised a method and filled a role that resonates with the interpretive approach, in that her epistemological model displaces the fixed subject–object relation with an iterative, interactive model.

As we see in Chapter 3, in her scientific work McLaren also relied on a method of modelling gene–environment interactions in the mouse across the fields of reproductive, developmental and molecular biology. These experiments presented analogies, she explains, for the same theoretical problem, and helped consolidate her understanding of this dynamic over the course of her career, filling out a model of how an organism gets 'from one generation to the next'. Here, as with McClintock according to Keller, we see another expression of an unconventional nexus between the conventionally dichotomous domains of knowledge and affect, tool and object of study. McLaren uses the scientific tools available to her to model an organism, building what we might call a feeling *as* the organism, using all the scientific tools she can from a variety of fields, communicating and translating the problem within these domains in order to build an understanding through assimilation of individual experiments into an expanding internal model – working with and through her object to build a scientific picture. McLaren did, after all, have her own computational influences, not only in her training with the ecological geneticist E.B. Ford at Oxford, but also as a result of her early experiments alongside Donald Michie, whose background in artificial intelligence as one of the leading code-breakers at Bletchley Park during the war is palpable in McLaren's methods of schematising her results in such studies

as the maternal influence on the number of lumbar vertebrae in mice in the early 1950s, and on. But these computational models were developed through a trained and intricate understanding of the biological system model at hand. Marylin Monk (2001), in this way, recalls how McLaren used to teach her students using blunt instruments and bad microscopes with suboptimal magnification, training them to work semi-blind so that the power they gained from suitable equipment, once they were assigned to a big problem, was magnified. She recalls,

> But Anne taught me that, by honing my own skills of dexterity and visual sense, I could observe more accurately than the most elaborate expensive machines and equipment. I learnt to work at the lowest magnification possible to 'leave room' for later more exacting work in the microenvironment of the embryo.
>
> Monk, 2001, p. 497

As for McClintock, in McLaren's style of practice, too, the tool is only part of a process that is centrally about the individual's capacity to build an understanding of the organism incrementally through cumulative experiments in which she exercised the same approach to modelling relations, integrating these into a growing internal model through a process of relating – results, people, tools – in order to read with and through the organism to arrive at a working model. McLaren, then, like McClintock, was practicing a different way of relating to her object through a hermeneutical process based on models that served as analogies, making explicit her interpretive lens and showing how the scientist is intimately involved with their object of study.

There is a sense in which I echo McLaren's approach in order to explicate her position. What I call a 'hermeneutics of practice' links McLaren's style of practice with my own method, which has in common the same assumptions, namely that repeatedly bringing an underlying understanding to a situation is transformative and that this effect is cumulative. By reading McLaren's texts across sectors, in the context of the HF&E debates, her scientific work and her political writing, I show how her life can be summarised as a pattern that results from her distinctive approach of feeling, communicating, connecting and facilitating within specific contexts. I show how this constituted a distinctive pro-research argument and how this provided a model for the relations between science and society more broadly. This close reading of individual acts of persuasion that accumulate to build a view of McLaren's style of practice allows me to trace her specific negotiation of scientific facts in a public debate to ask how tensions within these texts reveal modes of

thinking and doing that ran counter to certain dominant notions of 'scientific' at the time.

In combination with interviews that explore the legacy of McLaren and her policy contributions, I am able to ascertain to what extent McLaren was able to communicate, to persuade – *how* the relationship between science and society during the HF&E debates, of which she is an indexical case study, changed. I repeatedly bring this interpretive practice of close reading to the question of McLaren's style of intervention, which was itself based on a distinctive understanding, thereby offering a reading of a life through the lenses of cultural and political worlds, fulfilling the credo to "display the power of disclosure implied in" the author's "discourse beyond the limited horizon of his own existential situation" (Ricoeur, cited in Rabinow and Sullivan, 1979, p. 100).

McLaren's work in science, policy and politics provides another powerful example of the validity of a scientific approach that centralises the process of interpretation through feeling, communicating, modelling and conceptualising, and here also forms the basis of my sociological project, both substantively by documenting McLaren's approach, and methodologically by taking an individual's utterances as my point of departure in order to build a portrait of an era. It is in this sense that my readings of Keller allow me to bring under-theorised models for thinking about science as hermeneutical to light through McLaren, at the same time that it guides my thinking as I develop a new sociological model for explaining the evolution of scientific facts in public debate through the hermeneutics of practice – all by foregrounding the process of interpretation.

Theoretical background: Sociologies of translation

In the previous section, I introduced the guiding methodology for this dissertation, which is based on the insight that McLaren's style of practice was actually a broadly encompassing ethos based on an understanding of science as very much a part of society. Key to this approach in the context of the human fertilisation and embryology debates, was McLaren's use of the prospect of clinical translation in her arguments. The account of McLaren's conceptualising of the biological model in the wake of the clinical translation of IVF, as well the PGD in the making, revealed how McLaren used the prospect of clinical translation to build social consensus. The centrality of clinical translation in McLaren's deliberative process, and the ways in which this conceptualisation differs from contemporary 'pipeline' models that has come to characterise post-millennial policy rhetoric, made this a key thematic focus as I drew the lessons from my case study that might

be applicable to policy discussions today. This leads me in this dissertation to develop a model, drawing on McLaren's own language, for translation in the context of negotiating science policy today. I make a case for a broader understanding of the meanings encompassed by the sociological and scientific uses of the concept of 'translation' as the basis for a more clearly defined role for scientists in public deliberation.

My emphasis on translation has been inspired by the project, *Life in Translation*, led by Professor Sarah Franklin, Dr Noémie Merleau-Ponty and Karen Jent of the Sociology of Reproduction (ReproSoc) group at the University of Cambridge. This is a project that aims to broaden the scope of practices that are considered when describing the process by which "biology is brought into conversation with, directed at and converted into applications" (*Life in Translation*, University of Cambridge website, Accessed 15 August 2018). Whereas conventional definitions of translation used in science policy today emphasise bench-to-bedside, or research-into-practice flows of knowledge, this project explores the relationships between regenerative medicine and reproductive technologies, "the biotechnical and socio-ethical dimensions of translation between reproduction and regeneration paying attention to increasing emphases on inter-disciplinarity and public engagement" (ibid). Its interest is in the turn to dialogue in science policy and how this might invoke new relationships "between science and society", and to bring to light how translation can often be "more productively addressed through its misconceptions and misunderstandings" (ibid).

The case I describe in this thesis exemplifies many of these hidden connections and dimensions of translation. In this thesis, I contribute a set of 'translations' that emerge through the telling of an individual's role in public debate, as well as her biography. Here I will refer to two key sociological literatures on translation, in order to situate my thesis and my agenda as part of the project of bringing sociologists and scientists together over the shared problem of translation. I return in Chapter 4 to a discussion of the specific lessons that can be drawn from McLaren's role, to inform science policy discussions in the field of reproductive and regenerative medicine today.

First, I draw on translation as a paradigm that captures how concepts can connect researchers across labs and research groups, providing common, workable ground. This was a key theme in the literature on the sociology of translation of the 1980s. For scholars such as Michel Callon (1986) and Bruno Latour (1987), translation is an analytical category, used to describe how scientific objects, theories and evidence are made. Theorists

such as Joan Fujimura (1987, 1988) and Susan Leigh Star and James Griesemer (1989) have, in this vein, described scientific translation specifically as a process of negotiating coherence across disparate professional communities through shared conceptual or physical resources and practices that travel geographically and between fields in order to solve overlapping problems. McLaren, too, I will show, was particularly adept at finding common ground by developing conceptual models, as well as physical tools, as she travelled between domains of knowledge, disciplinary fields, and political regimes. Interestingly, she often does this by referring to clinical translation, by showing how it represents a set of values and feelings shared by various groups – scientists and potential patients alike. McLaren's case thus offers insight into the 'sociological' translation involved in arriving at 'scientific', clinical translation.

This emphasis on shared conceptual tools, can be used to explicate the priorities, aims, and ideologies contained in a fixed translational goal, by revealing the potential trajectories and research directions that are omitted, discarded or suppressed. In what follows, I offer various examples of the way in which McLaren made use of models based on a layering of perspectives to show how these overlapped over a shared clinical objective. McLaren, in this way, can also be placed in a tradition of biologists who believed in the power of visualisation, models, and paradigms, to reframe the questions in science. Most notably the embryologist and polymath C.H. Waddington, with his diagram of embryogenesis in the form of a 'visual metaphor' called the epigenetic landscape (Squier, 2017), served at first to connect scientists across embryology and genetics, but later, as the cultural critic Susan Squier (2017) shows, came to serve as a 'neutral epistemic object' across a variety of fields and professions. This reveals the efficacy of metaphorical thinking, of fluid concepts, in connecting and providing workable common ground.

As I describe McLaren's models and style of practice, I also come to offer my own translational models that serve as summaries of the thinking and language McLaren herself used. I introduce these in recognition of the ways in which these can be used to connect across fields, and entire domains – not just science and sociology, but also ethics, feeling, policy, and the law. Through this thesis, I hope to invoke scientists and policy-makers to replicate the kind of thinking that is in part reminiscent of the sociology of translation of the 1980s, and of McLaren herself, in finding shared languages. These concepts are often neither strictly scientific nor sociological, but hybrid and abstracted, and essential to setting clinical goals in a way that is representative, democratic and fair.

The way that ‘translation’ is understood by scientists today, is based on a discourse that emerged within the life sciences in response to a largely policy-driven, heightened sense of urgency about the delivery of tangible benefits for human health in the late 1990s. This approach emphasised the need for deliverables in what became known as the translational ‘pipeline’ and was accompanied by a host of pressures, tensions and confusion among laboratory scientists. Indeed, the first empirical research on clinical translation initiated by sociologists in science studies at the turn of the millennium emphasised the gaps that were experiences by stakeholders in the actual process of arriving at a clinical translation (see e.g. Cambrosio et al., 2006; Löwy, 1996). These studies explored the gap between the laboratory and the clinic that translation supposedly filled with a seamless pipeline model.

Interestingly, ethnographies of clinics and interview-based research have also been used to develop a new approach to public deliberation and bioethics. Wainwright and his colleagues, for example, explain how science and technology studies work by “exploring scientists’ views on the ethical issues relating to their research (e.g. Michael and Birke, 1994)” and showing “the ways in which ethical dilemmas and reasoning occur in the clinical setting” can provide models for the wider policy discussions of biomedicine, contrasting this approach to the “dominant, disembodied ways in which ethical reasoning is traditionally presented in philosophical bioethics (Haimes, 2002, Hedgecoe, 2004) and in philosophical science ethics (Resnik, 1998)” (Wainwright et al., 2013, p. 43).

The gaps between lab and clinic, brought to light by scientists themselves, and a systematic account of how these are negotiated inform not only a sociological account of knowledge production, but a sociological model for ‘doing ethics’ through collaboration. The paradigm of translation provides a common language between sociologists and scientists, as they map the trajectories and issues as they emerge in specific contexts. The field of regenerative medicine provides a particularly effective case study in this sense, as the very biological models being used to utilise technologies, such as stem-cells, reflect the complexity that the unilinear pipeline model does not accommodate. The language of scientists, as they describe these models in the context of translation, provides particularly fertile common ground for sociologists and scientists as they attempt to broaden the frameworks used to understand how the actual and potential connections between the lab and the clinic are formed in the negotiation of science and society.

Just as sociologists of clinical translation have done, I focus on the multiple and

contradictory nature of translation. Indeed, post-millennial sociological scholars of clinical translation have emphasised the complexity of the world of translational biomedicine leading to a need for conceptual and methodological expansion. Social analyst Michael Fischer (2012), for example, lists the various contexts in which translation occurs, thereby indicating the need for a broader conception of the term:

> Translation (across science fields and technological scales; from bench to clinic, or from green fingers to stable techniques and scalable production; and from experimental therapy to standards of care) and capital (financial, legal, symbolic, scientific) are the two linchpins of both the Alice in Wonderland worlds and the indirection of scientific opportunistic development. Both are lively, fluid, ever-moving.
>
> Fischer, 2012, p. 388

This is an ambitious project, requiring mixed methodologies and new concepts. I attempt, in this thesis, to map several of the shifting and multiple forms of translation as they arise in my description of Anne McLaren's role in the HF&E debates, thereby contributing to the interrogation of the process of translation by adding a detailed and textured account. Not only does this add to the multiple senses in which we can describe and schematise the process of translation, I also explore the role of the under-theorized 'human' perspective can play in such accounts, of how a detailed focus on the individual might relate to a systematic mapping of actors in the process of translation. This should be a welcome addition to the sociology of translation, considering that, as Fischer writes, "actor-network theory admits that the theory doesn't handle personhood or culture or people very well" (2012, p. 386). Yet this is about more than adding a 'human' side to accounts of clinical translation, as Fischer explains:

> This is not a call for journalistic "human interest" or "genius scientist" writing, but a meditation on the mix of detailing – organisational, historical, intellectual-genealogical, political-economic, material-technological, as well as the singularities of events, passions, and competitive bootstrapping – required to upgrade ethnographic work on the biosciences so that it can be in conversation with the understandings of practitioners.
>
> Fischer, 2012, pp. 386–7 [emphasis added]

I have devised a methodology that does just this – that moves between the macro-social, the individual, the historical and genealogical, the singular and the interactive, to develop a model that might be of use to both sociologists in their attempts to model translation, as well as to scientists, as a case in point of the role they might play in the

scientific-social-political-affective-ethical process of translation.

In my account, I expose the gaps described in the field of biomedicine above, as they appear in the process of moving from the Warnock recommendations to the HF&E legislation and beyond, in the legacy of the Act. Gaps characterise, I show, the process of arriving at consensus over a clinical application. Consensus is built on fracture, on partial agreement and alignment over a temporary common goal. By including in my understanding of translation both the emphasis on shared objects of the sociology of translation of the 1980s and the post-millennial emphasis on the gaps between lab and clinic – the glitches in the pipeline – I hope to show how both characterise the process of translation; the existence of gaps does not preclude consensus but defines it. Just as the sociology of translation of the 1980s showed how disparate elements were brought together to constitute knowledge, the sociology of clinical translation shows the gaps in the process of arriving at common ground. This understanding of the relationship between gaps and consensus informs an ethical model that can be applied to discussions on science policy in biomedicine today, as I argue in Chapter 4.

My account focuses on a time that precedes the rise of translational science in the 1990s; nonetheless it is useful to refer to this literature in my analysis because the questions that were being addressed in the context of the HF&E debates, the implementation of IVF – the place of research, the development of PGD – all raised similar questions and in many ways created the legislative environment for this translational turn in Britain in the context of reproductive biology and regenerative medicine (Banchoff, 2011). Moreover, questions regarding how legislation should respond to new clinical possibilities are just as pertinent today, with the discussions, for example, around the extension of the 14-day rule and the plethora of new clinical possibilities in regenerative medicine posing new regulatory challenges.

I draw on a case study from a time when clinical possibilities were being broadly discussed – also, unusually, by scientists in the public – as testament to the efficacy of combining sociological and scientific perspectives when thinking through science policy around clinical translation – in this case in the field of human reproduction. This is not just an argument for introducing sociological concerns or concepts into scientists' discussions about clinical translation, but like the researchers behind the *Life in Translation* project, I hope to show that sociologists and scientists share a common question with regard to translation. The question of how and why clinical translation today takes the shape that it

does for sociologists, and the question of how to negotiate these translational pressures for scientists, both revolve around the same central tension (noted in postmillennial sociological scholarship on translation) that has always existed – the gap between the toolkit available to the medical scientific community and the ongoing difficulty of curing human disease. 'Translational thinking', I will show, helps define the task of the scientist and the sociologist over a shared challenge that reframes the 'ethical' questions in discussions of biomedicine, in contrast to the bioethical emphasis on the status and value of biological materials, as a social problem. Positioning ourselves on the cusp between our current technological capabilities and projected, shared research goals defines the task of clinical translation for all those involved.

Chapter 1
Carrying forward the Warnock model: Translating the 'pre-embryo'

The first translation: Biological vision

In 1984, when the Warnock Report was released, the Government gave little indication of how they intended to act, seemingly still reluctant to dirty their hands on the controversial issue of research on human embryos, which had never been legislated on by any government (Edwards et al., 1993; Franklin and Roberts, 2006; Lee and Morgan, 2001). In the six years that passed between the release of the Report and government legislation in 1990, there were extensive parliamentary and public debates that, unlike the Warnock Report and its emphasis on infertility, focused predominantly on the issue of human embryo research (Mulkay, 1997). The difficulties of trying to fill a legal vacuum, as well as this governmental reluctance to act, characterised the staggered nature of the debate, as it moved through committees and bodies either indirectly set up by the Government or independently initiated in lieu of any official response. This unfolding also saw the gradual awakening of embryologists to their role in the debates. Many of the scientists I interviewed, who were active alongside McLaren on the pro-research lobby, described to me how they became aware of the extent of the public hostility to research, which they saw as primarily the product of inadequate understanding of early embryonic development (e.g. Gardner and Graham, interview with author, 27 September 2018; Pembrey, interview with author, 17 October 2018; Winston, interview with author, 26 September, 2018). The discussions between scientists and non-scientists in the debate were thus initially problematized by ignorance on both sides – both by the scientists' lack of awareness about the state of public knowledge, and the non-scientists' lack of knowledge about the events of pre-implantation embryonic development.

This bilateral myopia provided the background against which communication would have to take place in order to reach a consensus understanding of the entity initially at the centre of the discussions – the human embryo. In developing a case in favour of research, the pro-research lobby was initially hard-pressed to find a convincing alternative to the dogmatic stance taken by pro-lifers that asserted that life began at conception, and that any research on human embryos was, therefore, immoral (Mulkay, 1997). The pro-

research rhetoric eventually became characterised by a term that McLaren introduced into the debates – the 'pre-embryo'. While the term itself was highly contested and eventually disappeared from official legislation as well as embryological literature, it acts as a nexus for the broader contestations around the role that scientists and scientific information would play in the pro-research case.

My focus on McLaren's particular role in developing the case for the 'pre-embryo' introduces some important nuances into previous analyses of the term (e.g. Crowe, 1990; Deech and Smajdor, 2007; Franklin, 2013c; Gunning and English, 1993; Jasanoff, 2005; Lee and Morgan, 2001; Spallone, 1987; Steinberg, 1990; Strathern, 1992). While many scholars of the debates have shown how the term was key to the organisation of a pro-research lobby (Birke, Himmelweit and Vines, 1990; Mulkay, 1994), in these analyses, the 'pre-embryo' is usually presented as part of a reactionary argument, merely providing an alternative locus to which to attach the same values, and the special status of early human life that the anti-research lobby ascribed to the human embryo from conception. My analysis shows that the term was indeed part of an iterative process of rallying support in favour of research, but that, in doing so, the biological model denoted by the term 'pre-embryo' represented a set of facts that was used to make a case in favour of research that was justified in terms of the good it could do for society.

The pro-lobby case, therefore, did not hinge on the ontological distinction between 'pre-embryo' and embryo-proper, but on a systemic ethical argument, instantiated by Mary Warnock's framing of the debate in the Warnock Report (1984). This social way of framing the question at hand differed vastly from the model of the anti-research lobby that revolved around defending the rights of the embryo as an abstracted entity. Indeed, the history of McLaren's use of the 'pre-embryo' allows me to trace a shift in which the biological model becomes increasingly relative to a set of clinical applications that are framed and defended in terms of the good they can do to society, denoting a different role for both scientific information, and the scientist herself, in public debate. In this chapter, after outlining the approach taken by Warnock and describing McLaren's contributions to the Warnock Committee, I will reconstruct the role that the scientific narrative played in the pro-lobby case by referring to my interviews with the scientists involved, as well as through a series of close readings of the public information material produced by McLaren over the course of the debates.

The Warnock embryo

The Warnock Committee, in 1982, faced the unprecedented task in the context of UK policy-making, of examining the "social, ethical and legal implications of recent and potential developments in the field of human assisted reproduction" with an eye to advising Government on legislation (Warnock, 1985, p. vi). As Mary Warnock stressed in an interview with me, she saw this as a problem of defining the embryo:

> … an embryo alive in a dish in the laboratory was an entity that had never existed before, so there was absolutely no way that one could find any analogy from any other ethical dilemma. So, it was quite frightening actually, having to lay down the law based on what was morally acceptable with regard to an entity that had never existed before. And it was really, branching out into completely new territory at that time.
>
> Warnock, interview with author, 21 August 2018

Warnock thus saw her most difficult task as the ethical[1] and legal problem of defining the embryo as an entity. As it turned out, she took a tack for which she was criticised by philosophers, even the so-called 'applied' philosophers, who, unlike the meta-ethicists, believed that philosophy could incorporate "description of the complexities of actual choices and actual decisions, and also discussion of what would count as reasons for making this or that decision" (Warnock, cited in Wilson 2014, p. 143). When it came to advising the Government on the status of the embryo, Warnock saw limitations to every philosophical approach, arguing that even the philosophical position with which she was often associated, utilitarianism,[2] would not in itself solve the issue of what the embryo is,

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1 I use the terms 'ethics' and 'morals' in the first instance to reflect their use by Warnock and McLaren themselves throughout the Warnock discussions and the wider debates. Warnock has confirmed in personal correspondence that their use of the terms did not reflect any particular philosophical distinction but was used colloquially: 'morality' more generally with reference to individual behaviour rather than group behaviour or public measures. 'Unethical' and 'medical ethics' were, for example, used as opposed to 'medical morality'. I later develop the sense of an ethos to describe a model for ethical policy-making. Here, my use of the term 'ethics' is closer to that of sociologist Charis Thompson in her book Good Science (2013), where ethics refers to the "activities … in which various actors … advocate for some way of proceeding with … research over others on the grounds that they would be better for some people or things in some way" (p. 26).

2 Generally, I use 'utilitarianism' to refer to the philosophical position, and 'pragmatism' to describe the approach Warnock took to justifying the 14-day rule, which, as Warnock (1985) has written, and as I show in this chapter, was utilitarian up to an extent but also 'sentimental', in that it the committee deemed research

as this decision would always require ethical feelings, as well as rational arguments. As Duncan Wilson (2014) suggests in his assessment of Warnock's contribution to the development of what he calls 'British bioethics', this meant that, for Warnock, "a philosopher could help to clarify the properties and consequences of a particular moral standpoint" but "could not prove or otherwise show conclusively that one view is to be preferred to another" (p. 168). She took instead what might be called a sociological view, arguing that disagreement was "unavoidable'" in a pluralistic society that always lacks "an agreed set of principles which everyone, or the majority, or any representative person believes to be absolutely binding" – especially in "areas of moral concern which are radically and genuinely new" (Warnock, 1985, p. xi). She therefore also opted for a kind of sociological pragmatism that aimed to find a resolution that would be acceptable to most people so that the law would reflect the "minimum requirement for a tolerable society" (Warnock, 1985, p. 3). This approach diverged from contemporaneous attempts to make recommendations for the regulation of IVF and embryology, such as the Glover Report (Glover, 1989), presented to the European Commission, which foregrounded individual choice and freedom, as opposed to the social problem that is the organising principle in the Warnock Report.

Warnock received criticism for her 'compromise solution' from philosophers, opponents and proponents of research alike. Warnock was convinced from early on, however, that supporting scientific progress in aid of new clinical applications was morally right. She nonetheless also believed that this would entail setting strict limits in law that would not be crossed. She intended to do all this while avoiding the question of embryonic status, which, especially given the irreconcilable 'ethical feelings' referred to, she saw as a dead end. Instead, she reframed the question in line with her philosophy and her moral conviction, as one of how to treat the embryo, or, "at what stage do you have to treat the human embryo as needing to be protected in the way that one treated the child that had been born as needing protection?" (Warnock, interview with author, 21 August 2018). The

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desirable because its potential benefits were so great, but that the decision that research should be capped at 14 days was based on the sense that the connection between these cells and a future baby is "extremely remote" and therefore "based on their own feelings and those expressed in evidence", and so also based on a broadly shared sense of morality, "once again, social, an idea of social morality" (pp. 517–518). I describe later how this pragmatism can also be regarded as distinctly sociological.

Warnock investigation thus became one of establishing limits to practice based on solid justifications, as opposed to definitive bans or any claims to the status of the embryo *ipso facto*. This pragmatic framing of the question thus required a solid, scientific case for a proposed set of applications that incorporated biologically justifiable limits that could be translated into law; Warnock needed what McLaren eventually referred to as 'landmarks', or transition points, in early human development that could provide a rationale for a broadly applicable set of regulations. This meant that Warnock came to rely heavily on McLaren, who was the only practising embryologist on the Committee. As we shall see, the case that was developed primarily by Warnock and McLaren and then later carried forward by the pro-research lobby following the publication of the Warnock Report, depended on two central translations, enumerated to reflect their order of precedence in the case developed by McLaren. The first was the translation of biological facts of early embryonic development into a socially meaningful account. The second was a translation of this biology into clinical applications – a demonstration of the possible interventions into this process that could improve on natural reproduction, that itself, as we shall see, required another set of translations.

McLaren's role on the Warnock Committee, then, was one of providing a biological rationale for legislation. As it turned out, though, this required more than imparting a standardised description of early embryonic development. McLaren's account of the facts of life was from the onset attune to the Warnock framing – of how the embryo should be treated in a tolerable society. In the account McLaren provided for the Committee, she made a case for thresholds in biology based on an awareness that these were drawn for social reasons. Her biological account was therefore never only biological but was framed by a legal and sociological question. This is evident in the case she developed for the so-called 'pre-embryonic' period of development; not only the arguments she provided in support of the distinction, but also how she used her role as a scientist to build an allegiance around it.

First, I will describe how McLaren delineated the 'pre-embryonic' period in her biological account for the Committee. I will then move on to show how McLaren carried this term forward post-Warnock Committee, and how this reflected her evolving understanding of her role as a scientist in the public debate.

Social framings: Landmarks in a continuum

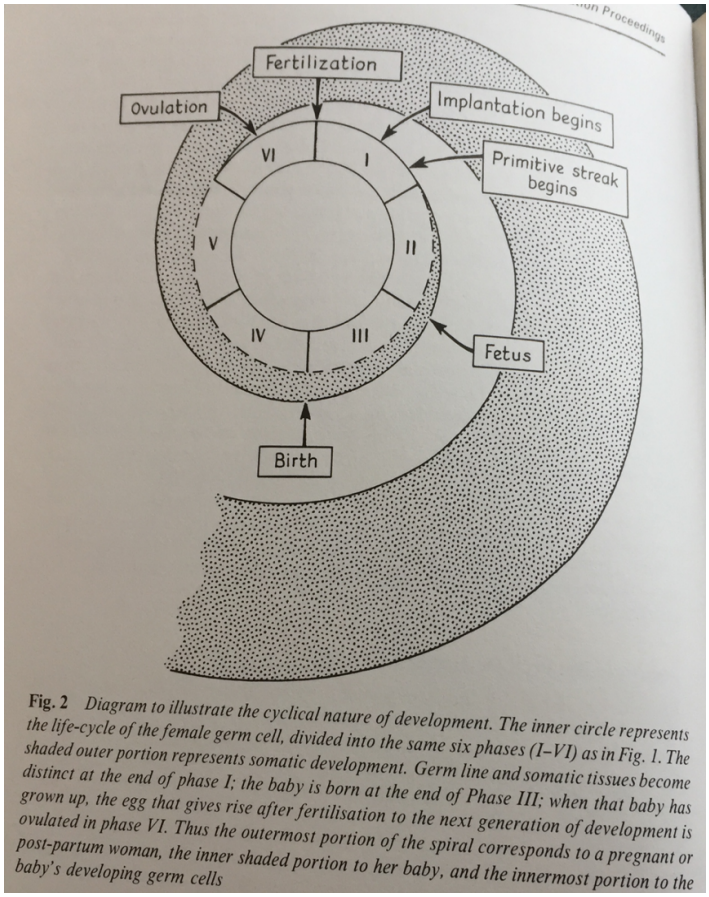


Fig. 2 *Diagram to illustrate the cyclical nature of development. The inner circle represents the life-cycle of the female germ cell, divided into the same six phases (I–VI) as in Fig. 1. The shaded outer portion represents somatic development. Germ line and somatic tissues become distinct at the end of phase I; the baby is born at the end of Phase III; when that baby has grown up, the egg that gives rise after fertilisation to the next generation of development is ovulated in phase VI. Thus the outermost portion of the spiral corresponds to a pregnant or post-partum woman, the inner shaded portion to her baby, and the innermost portion to the baby's developing germ cells*

Figure 1. The Spiral. In "Where to Draw the Line", *Proceedings of the Royal Institution of Great Britain, 56*, 1984, p. 106.

In this diagram McLaren schematises embryonic development as a process that extends across the generations, encompassing the pregnant mother, her baby, and the baby's developing germ cells. She uses the diagram to show that development is a cyclical process in which any line that is drawn will, therefore, be biologically arbitrary and will always rely on social, legal and ethical considerations to be meaningful. McLaren makes less use of the cyclical argument as the human embryology debates progressed, opting instead for linear diagrams that helped her make a stronger case for the ways in which the particular transition in development at the primitive streak was socially, ethically and legally meaningful, and that would come to substantiate the arguments for the 14-day rule.

The paper, *Where to Draw the Line*, was published in 1984, but an earlier version presented by McLaren to the Warnock Committee had provided the biological rationale for legal lines that Warnock had sought. As Warnock stated in an interview with Sarah Franklin and Martin Johnson,

> [W]hat Anne had provided us with was a kind of rationale, I mean we could justify picking on that particular day, at 14 days, because of what Anne had taught us about the development of the embryo and the date after conception at which differentiation began. And once we'd got that into our heads, then in a way, everything flowed from that. ... [O]nce you got a regulatory line beyond which, if you passed beyond which, you committed a criminal offence, then you needed some justification for having the line – the essential thing was to have a line. And so we didn't say anything like that the embryo before 14 days or fifteen days, was completely different from the embryo after the fifteen days, we just told the story of the development of the embryo, the appearance of the primitive streak, the subsequent differentiation, and the fact, too, that identical twins could form up to fifteen days, all that – there's a sort of combined rationale for having put 14 days as the time.

Warnock, interview with M. Johnson and S. Franklin, London, 11 June 2008, Mammalian Developmental Biology Interviews, British Library

In this paper, McLaren makes explicit the extra-scientific considerations that structure the account of embryogenesis used by the Committee – the need for laws and limits that represent consensus in a society. As the Inquiry moved forward with its recommendations, as Warnock indicates, this social framing of the biological narrative was left much more implicit and the distinctions it makes are increasingly presented as objective biological facts – evident in the increasingly common use of the new scientific term 'pre-embryo' – to denote the mass of cells up to the emergence of the primitive streak around 14 days after fertilisation.

However, McLaren helpfully lays out the reasoning underpinning the divisions that she made in the biological narrative (For an analysis of this paper, see also: Franklin, 2019). She opens the paper by discussing the relationship between biology and 'landmarks'. The landmarks she considers are "birth" and "infancy, childhood, adolescence, adulthood", which never map perfectly onto biological change – their relationship differs depending on the individual or the biological system being studied, and they are "arbitrary" as biology is in fact a "continuum" (McLaren, 1984, p. 102). McLaren thus describes the social basis of these landmarks in development after birth; they reflect biological events, but these categories only acquire their meaning in relation to specific questions asked about biology and at best reflect a population average. She then notes that the lines drawn in law, as opposed to biology, are expressed "in terms of chronological age, rather than in terms of biological landmarks" but that, "given that developments in biomedical science make the embryo more accessible to interference", this will be necessary in the case of early embryonic development (ibid).

McLaren thereby frames the problem that she is trying to address with her biological narrative as one particular negotiation of the relationship between biological facts and the law. She has also indicated how this negotiation might be conducted, namely through a process of layering social concepts onto biological information – a superimposition that will never be seamless but is unavoidable when trying to identify "landmarks in the continuum of biological development" (p.101). The description of early embryonic development that follows is accordingly structured around several socially relevant landmarks. These criteria are "uniqueness, which is a genetic phenomenon; the addition of new genetic material; individuality, which relates to the organism as a whole; brain function, which is necessary but certainly not a sufficient condition for consciousness; and viability" (p. 107). McLaren then opens her biological account by describing the early formation of germ cells that precede the formation of the embryo. She explains,

> When you look at a pregnant woman, you are actually looking at three generations: the woman herself, the baby in her uterus, and the germ cells inside that baby's gonads, which are already well-developed and playing their part in the germ-cycle.
>
> *Where to Draw the Line*, 1984, p. 107

Against the backdrop of this continuous biology, she goes on to "clothe" the "potentially immortal germ cell cycle in the trappings of mortality" to describe those aspects that might provide meaningful landmarks (p. 105). This point comes at "about three weeks after fertilisation when the primordial germ cells can first be identified" and "we can also identify the progenitor cells of the other main systems in the tissue layers of the body" (p. 105). It is only now that, "for the first time we can say with certainty that the descendants of certain cells will become nervous tissue, others will become gut … and so on" (p.105). She depicts this point on a spiral diagram showing how the "germ cell lineage is divided off from the rest of the embryo" (ibid) (see figure 1).

McLaren only uses a dotted line to separate the germ line from the embryonic soma, however, to indicate that there are still "many interactions that occur between germ cells and somatic cells throughout life", offering another sense in which the distinction being made is, biologically, arbitrary. McLaren, in the rest of her paper, goes on to discuss this point in development, at which the germ layers divide, in relation to the criteria outlined above, showing that it is only at this point that the rudiments of a unique and individual entity can be identified, while the criteria of brain function and viability are still not satisfied. This set of factors substantiates a distinction she makes between the 'pre-embryo'

or ‘early embryo’ for the first two to three weeks, and the ‘definitive embryo from then until eight weeks. The “early embryo”, she writes “includes all the cells derived from the fertilised egg, but the definitive embryo … includes only a fraction of these” (p. 106).

Of the potential criteria listed, then, individuality is foregrounded as the most relevant to the question of how the embryo should be treated in research. The ‘embryo’ demarcates the emergence of a new individual body, while the ‘pre-embryo’, although genetically unique, is composed of both potentially embryonic and extra-embryonic tissue and is, therefore, physiologically, undetermined. Here then, is the ‘combined rationale’ that Warnock describes: in response to a need for regulatory lines, using a set of social criteria mapped onto biological landmarks, and against a backdrop of a picture of biology as a continuum, McLaren implies (she is careful not to make her ethical claim explicit) that research on ‘pre-embryos’ would precede the biological-social-legal amalgam that is individuation.

Arbitrariness

Allegations of ‘arbitrariness’ to discredit the term ‘pre-embryo’ as a distinction in development frequently appeared in the discussions, especially following its official introduction into the pro-research lobby’s rhetoric after the publication of the Warnock Report (Davies, 1986; Chargaff, 1987; Kelly, 1990; Short, 1987). McLaren herself also uses the term ‘arbitrary’ several times in *Where to Draw the Line* to stress the gap between the biological account given and the social and legal work of drawing lines. She writes, for example,

> I have tried to give a picture of human life as a continuous process, punctuated by landmarks. I have not tried to answer the questions that are listed on page 102 [When does life begin? When does an embryo become human? When does it become a human being? When does it become a unique individual? When does it become conscious?] … *any lines that you draw are going to be to some extent arbitrary, and where you finally draw decide to draw them must depend on the context, the purpose, of your decision*.
>
> *Where to Draw the Line*, p. 117 [emphasis added]

The arbitrariness of the cut-off point here refers to the fact that it cannot be derived directly from the biological account but is relative to the question being asked. ‘Arbitrary’, to McLaren, then, really means that the distinction being made cannot be understood ‘scientifically’ or ‘biologically’ – on its own terms. In this passage, McLaren points to the

fact, however, that any line drawn in biology in order to legislate will inevitably be arbitrary, given that human life is a 'continuous process'. The scientific vision of development by definition will always need to be moulded to a specific context in order to draw any lines at all. Drawing on the biological picture in order to devise legislation will always require a translation of the biological vision of continuous development into an account that responds to a specific, socially meaningful, question.

I have chosen to call this translation of a biological vision the 'first translation', because this is where McLaren becomes instrumental to the Inquiry. Nonetheless, by beginning my account with the biological facts used in the Warnock case, I have also shown how these were, from the start, incorporated into a framework already established by Warnock, and by a scientist who was well aware of this social, legal framing of the question at hand, making them not primary in any theoretically meaningful way. Biological vision, as a translation then, encapsulates the sense in which McLaren recognised the continuity of development, but then drew on this scientific view to respond to a specific, socio-legal question. The translation, as figure 1 shows, leaves her biologist's vision of continuous development intact, but draws from this the biological information that becomes socially meaningful when mapped onto lay perceptions of development that prioritise, for example, the point of individuation. This amalgamation of the biological and the social accounts of development can then be used to substantiate legal cut-off points that are likely to recruit wider-spread support now that the biological account itself seems to reflect understandings and priorities representative of those held beyond the scientific community alone. The biological translation is actually a social translation.

The scientist as translator

The role of the scientist in this model, is not to unilaterally impart what they know to an under-informed audience, but to respond to social and legal criteria by drawing from their repertoire the relevant information, through a deep understanding of the question at hand. Nonetheless, in lieu of the more public debates on human embryo research, McLaren here seems still reluctant to claim a role that extends beyond the scientific literalist paradigm of 'informing'. She presents at the end of her text a disclaimer that she has not tried to make ethical deductions from the biological account, suggesting that McLaren is staking out a narrowly defined role for herself as a scientist in these policy discussions. The question of 'context' or 'purpose', she writes here, fall outside this remit as a scientific adviser. Despite

having explicitly factored social considerations in her biological rationale she distances herself form an ethical position, however heavily implied it may be in her biological account. In terms of figure 1, McLaren claims the inner circle, the 'picture of human life as continuous' as her area of expertise, choosing to 'clothe' this circle in the 'trappings of mortality' to inform the policy discussions, but retaining a firm separation between these perspectives.

However, as McLaren continued to participate in the broadening discussions on human embryo research, taking the biological picture presented to the Warnock Committee to a wider constituency, we see how she increasingly saw it fit to comment on the 'context' or 'purpose' that gives meaning to these arbitrary biological distinctions. She incorporated this broader, social commentary into her scientific remit by describing the relevance of her biological account to clinical applications in reproductive medicine. In doing so, the circle at the centre of her embryological spiral diagram receded increasingly into the background as she made a more definitive, authoritative statement on how early human development should be exploited, centred around the concept of the 'pre-embryo'. It is in this sense that I refer to McLaren's use of the biological facts as a 'translation'. Not only do I use the term to foreground how the biological model was already a translation of social concerns, but I also do this to describe the process by which meaning is constituted. The biological facts in the account I have given incorporate not only the biological facts as they are known by scientists given the state of knowledge, but their very induction into the debates were also always adapted to the specific question posed by the Warnock Committee, and become part of a context-specific negotiation of the facts in the process of emphasising the elements of the account that resonate within the framework of goals that reflect wider public interests.

McLaren is in this sense less concerned with imparting facts but instead aims to recruit the public to a shared 'biological vision' that requires a model that incorporates their concerns. The 'facts' that communicate this biological vision to scientists, might not transport the same meaning to a person with no scientific training. Rather than a literal transplanting of facts, this view of translation is closer to the conceptualisation taken from the humanities. In the poststructuralist theory of translation, a 'sense for-sense' translation is preferable to a 'word-for-word' one (see Derrida, 2001). A good translation will not transplant, literally, an account into a different context, but will translate a 'sense' of its meaning.

Non-literal translation

The poststructuralist philosopher Jacques Derrida has written extensively about this 'sense-for-sense' theory of translation. Derrida's theory of translation can be explained in terms of his now famous critique of the linguistic sign embodied in his concept of 'différance'. Meaning, for Derrida, is not contained in any singular utterance, but is an effect of the relations and differences along a potentially endless chain of signifiers. Meaning is always differential and, therefore, deferred: "always already a site of proliferating possibilities that can be activated in diverse ways by the receivers of that utterance" (Venuti, 2003, p. 240), according to their particular context in the web of signifiers in which they are embedded. It is this differential that gives language its vitality, it is only through its relationality that meaning emerges. In this sense, any utterance is also a translation, a product of the transposition of a term into a given context of signifiers.

To the biological metaphors embedded in expressions like 'native language' and 'mother tongue' that imply that language has somehow natural, unitary origins, Derrida proposes alternatives that emphasise the hybrid, cultural origins of language that functions as an organism, as Sarah Franklin shows:

> Thus he understands writing as a living system comparable to an organism, with a kinship to other technologies, for which one of his many analogies is grafting. In order even to begin to read a written text, one must become enjoined with its physiology, he claims, just as the text itself is the live offspring of previous couplings … He reconstitutes the reader's living relationship to the written text "as an organism, indefinitely regenerating its own tissue behind the cutting trace, the decision of each reading" (1981: 63). For Derrida, writing is not dead or barren because it is a copy of an original, but instead more lively because it is recombinant.
>
> Franklin, 2013a, p. 304

Franklin goes on to show how the technics of language mirror the process of the embryologist, whose results are often the product of a specific set of technics, rather than a free-standing, objective 'reality' – the language of biology, as well as biology itself, is alive by virtue of its relation to a broader context of relations, to the technics of scientific tools as well as language, as well as legal provisions, ethical frameworks and affect. We cannot speak of the scientific 'truth'; indeed, we cannot speak of biology without acknowledging these relations. Franklin also shows how Derrida's physiological metaphors resonate with the analogy used by anthropologist Marylin Strathern (1992b) to describe "merographic thinking – through which a partial connection 'from another angle' can

displace one meaning for another – a process she describes as substitution in her critique of the metaphysics of nature and culture" (Franklin, 2013a, p. 304).

Scientific facts, in this view, are never definitive, because the language used to express them is only ever a partial perspective that signifies meaning at the same time that it changes the subject in question. As in Derrida's model, meaning is never stable, is always in the process of being negotiated through language. It is in this way that the language used to discuss biology, the way in which facts are translated into new contexts and new domains, change and substitute the ideas people hold about the world and thus what is deemed reality. What is at stake in the relationship between biology and language is the categorisation of biology itself. This relationality of language, the connection of any idea to a host of others, leads Strathern to posit a moral imperative to take seriously the language we use to discuss the biological facts of life:

> This makes evident one of my starting points: that it matters what ideas one uses to think other ideas (with). Reproduction concerns everyone. Yet when human beings reproduce themselves, they in inevitably do so with already existing and thus specific forms of themselves in mind.
>
> Strathern, 1992, p. 10

It is only, then, by acknowledging the relationality of biological facts, that we can begin to translate them consciously into discussions – both professional and public – to move towards interpretations of the facts that are representative of the ideas and values of a broader social identity.

The second translation: Clinical applications

Following the publication of the Warnock Report, McLaren continued to play an active role in making the case for the continuation of human embryo research. Immediately after its release, she went on to be an important member of the Dawes Committee set up by the Medical Research Council (MRC), on the Voluntary Licensing Authority (VLA) set up in lieu of government action on the Warnock Report, and as an influential voice in the various lobbying organisations – most notably Progress. The continuity of her involvement and her authority as an eminent scientist, head of her own MRC Unit, and member of the Royal Society, meant that she was able to exert considerable influence on all of these bodies. The breadth of her involvement, we shall see, was reflected in the breadth of her vision and understanding of what would be required of the scientist and of what would be considered

'scientific' information in the debates, especially as she began to comment more explicitly on how the 'pre-embryo' could be used on the path to clinical translation.

McLaren's correspondence taken from her archives at the British Library provides one example of how she used her position on the Warnock Committee to drive an effort to build a scientific case around the now legally delineated period of the first 14 days of development. I found these papers during my scan of McLaren's extensive archives that have been deposited at the British Library in two tranches over the past years. I am the first researcher to be working with them towards a larger project. The papers show that, in the lead-up to the publication of the Warnock Report, published in July 1984, the archives reveal that McLaren wrote to Dr D.C. Evered at the CIBA Foundation on 15 March 1984, to suggest organising a conference on the subject of human embryo research. She writes,

> I am increasingly feeling that there is a need for a meeting to discuss the issues involved in research on early human embryos. I am not betraying any Warnock confidences by telling you that this is a very contentious issue on which there is a *great deal of confused thinking and public concern*. What I have in mind is something along the lines of the 1973 "Law and ethics of AID and embryo transfer" symposium, which as you know has had a not inconsiderable influence over the years. … If you looked favourably on the suggestion, would there be any chance of fitting in a short meeting, say 1 to 1½ days, in early 1985? This would be good timing in that the Warnock Report would have died down, but the Government would presumably not yet have got around to formulating any relevant legislation.
>
> McLaren, 15 March 1984, Anne McLaren Papers, British Library, Add MS 83887, [emphasis added]

The letter shows that McLaren saw it as paramount that scientists provide a consistent and clear pro-research case to inform a public and parliamentary debate that would precede any action taken by the Government. Clearly, McLaren saw this as very pressing, even before the much-cited vote on the Powell bill had revealed the extent of the opposition to research. After an initial rejection, McLaren writes to the CIBA Foundation again on 13 September,

> As you will realise from the Warnock Report, embryo research was the most contentious area that was dealt with, and one on which the Committee was deeply divided. It was only by the narrowest of margins that the fertilisation of donated human eggs for research purposes was approved. The earlier report of the Council for Science and Society, which you mentioned in your letter to me as having covered much of the ground that I had in mind for the meeting, in fact only dealt very briefly with the use of spare embryos for research and did not even mention the possibility of using donated eggs fertilized for research purposes … Obviously there will now be a lag of a year or more before any legislation is drafted and it seems to me that *legislators should have some reliable source of information in this area which is at*

> *present so liable to misunderstanding … The risk of course is that legislation could be passed which would hold back both our understanding of embryonic development in these important early stages, and our ability to alleviate infertility and genetic disorders. …* I envisage such a meeting lasting for say 1½ days, with ample time left for discussions. I enclose the kind of programme I have in mind.
>
> British Library, 13 September 1984, Anne McLaren papers, British Library, Add MS 83887 [emphasis added]

McLaren makes the case for a conference and a specific agenda. She insists on the importance of tackling separately and explicitly the question of research. In doing so, McLaren envisions a conference that not only provides a reliable source of information, but also makes explicit the need for research by linking it to clinical translation. This aim is implemented in the following draft agenda:

1. Introduction
   Fertilisation and the early stages of embryogenesis
2. Infertility
   Nature of the problem, and clinical management
3. Genetic disorders
3.1 Nature of the problem, and types of genetic disorders
3.2 Research possibilities
3.3 Possibilities for therapeutic intervention
4. Other issues:
   Other areas where research would be fruitful, and areas where it would not be fruitful
5. The ethical acceptability of embryo research:
5.1 Moral arguments for and against, and the problem of assessing "public morality"
5.2 Ethical status of the foetus in world religions
6. Conclusion
   Major points of agreement and disagreement

Figure 2. Draft agenda CIBA Symposium, 1985. British Library, Anne McLaren Papers, Add MS 83887.

Anne McLaren's proposed agenda for the 1985 CIBA conference offers a condensed summary of the relationship between basic scientific research on the early embryo and the goals and applications in the area of fertility and genetics. The agenda shows how McLaren carries forward the 'utilitarian' Warnock model that connects the biological facts to a specific therapeutic problem via basic research. Infertility, genetic disorders, and other issues both follow this structure. McLaren goes on to explain how she hopes the conference would serve to re-focus the discussion from questions of moral status to what she calls 'scientific aspects':

> The aim of this meeting was to provide a forum in which scientists, moral philosophers, doctors and lawyers could discuss together the issues raised by

> research on the early stages of human development. This topic has been subject to much public debate in the UK: most of the debate has centred on moral issues, and little information has so far been made available on scientific aspects, including research needs and research possibilities. We hope this volume will provided new insights as well as relevant facts.
>
> McLaren, 1985, British Library, Add MS 83887

Just as in the Warnock Report, she frames the issue as one of finding morally permissible applications, here in the form of therapeutic intervention, in the interest of moving discussions away from questions of moral status. The aim is not to counter the fundamentalists who will not accept research under any circumstances, but to inform those who believe the human embryo could, in principle, be used for research provided that the outcome is morally desirable. As Warnock quotes a philosopher as saying in her report, it is about finding a "steady and general point of view, that reflects an idea of a good society, to which most, but not every individual would agree" (Warnock, 1985, p.1). McLaren realises this aim by building an alliance around a research agenda geared towards a set of clinical applications. The final item of her draft agenda betrays the central purpose of the meeting – to build consensus by tallying agreements and disagreements. Through discussions of science a view to which the majority could agree was to emerge, based on a scientific research agenda and a set of therapeutic goals.

It is clear here that the agenda McLaren proposes is supposed to encapsulate what she perceives to be the value of clinical translation expressed in a specific relationship between basic research and clinical application. Her active role in attempting to frame the wider debate on human research through the conference, and by proposing a specific structure and agenda, shows how clinical translation was a very deliberate goal, articulated in the context of political debate, that started as a broad research orientation, and then, through the work of collaboration, coordination and unification that conferences like this facilitated, increasingly came to represent both real clinical outcomes but also, as we will see, a consistent 'biological' case in favour of research. The work that McLaren does in devising the conference could thus be described as another form of translation, necessitated by a clinical outcome, namely the work of coordinating and framing for the professional community of scientists, of building a research agenda, thereby pragmatically implementing her objective of communicating the value of clinical translation by organising a conference to facilitate it – a form of professional translation. This is the kind of work that, as sociologists of clinical translation have repeatedly argued, accompanies the

linear bench-to-bedside models espoused by research grant providers and policy initiatives. A description of McLaren's role in the HF&E debates offers another multi-layered account of the work involved in securing this translational pipeline – less a pipeline but a tree maintained by a network of roots that represent a whole set of invisible translations.

The newly consolidated scientific case also comes with the introduction of a new scientific term. The talk that McLaren gave at the CIBA meeting is contained in the report published in the following year under the title *Prelude to Embryogenesis* (1986). Here she introduced and offered a rationale for the term 'pre-embryo' explicitly, which begins to show how her use of this term was linked to an increasing sense of clarity about her role in the discussions:

> At this meeting we shall be mainly concerned with the first two weeks of development, the 'pre-embryonic' stage …The CIBA Foundation has organised two previous symposia on early development. The timing of those meeting was significant. The first, on Pre-implantation Stages of Pregnancy in 1965, was held towards the end of a pioneering period of studies of mammalian development, when *in vitro* culture and manipulations such as the production of aggregation chimeras were new and exciting. The terms 'embryo' and 'ovum' were at that time used interchangeably. The decade that followed saw an explosive rise in research on pre-implantation stages in mice, In 1975 the time seemed ripe to push forward into the technically more complex postimplantation period, so the symposium on Embryogenesis in Mammals made an attempt, only partially successful, to focus attention on postimplantation development … it was while I was trying to put together for the 1975 symposium a growth curve for the whole of mouse prenatal development that it first began to dawn on me that the 'embryo' as a continuous entity could be traced back from birth only as far as the primitive streak stage … and that the 'embryo' that develops from fertilization onwards is a different entity, which includes and gives rise to the 'embryo' that grows into a foetus and neonate but is in no way coextensive with it. It has taken a further ten years and some pressure from outside the scientific community for this distinction to result in a suggested change of terminology to eliminate the ambiguity of the term 'embryo'.
>
> McLaren, *Prelude to Embryogenesis*, 1986, pp. 14–15

McLaren's brief history of CIBA symposia on embryogenesis links the adoption of new terminology to an increasingly refined and more complex picture of embryonic development. By placing the introduction of the term 'pre-embryo' in this history of evolving research agendas in embryology, she suggests that it is meant to delineate a phase in development that is of particular interest to researchers and should incite additional research into this little understood time. The use of the term is here justified purely in terms of setting a research agenda. In the context of a symposium organised for scientists but very much in light of the need to inform the public debates on human embryo research, McLaren

also no longer uses the topographical language of landmarks that she did in *Where to Draw the Line* and there is no mention of ethics or social concepts. McLaren made a point in her conclusion of distancing herself from the role of the ethicist:

> Where to draw the line involves ethical judgements, which I have been careful not to make. But these ethical judgements can and should be based on a correct understanding of what is actually going on at the scientific level. I have tried to give a picture of human life as a continuous process, punctuated by landmarks. … any lines that you draw are going to be to some extent arbitrary, and where you finally decide to draw them must depend on the context, the purpose, of your decisions.
>
> McLaren, *Where to Draw the Line*, 1984, p.117

Despite this reluctance to make ethical claims, McLaren presented her biological account as informed by a social understanding of landmarks, and with an eye to legislating. Now, however, McLaren no longer emphasises the 'arbitrariness' of the limit she espouses, given that it is substantiated by a proposed scientific agenda which, we shall see in the following chapter, also makes the embryo an increasingly real and researchable entity through its translation into tangible clinical outcomes. It seems that McLaren now commits herself to an ethical position by proposing a 'context' and 'purpose' that she, as a scientist and an individual, deems important, thereby taking up her own challenge in *Where to Draw the Line*. Now, given that the talks following her *Prelude* provide examples of the outcomes of studying this phase in development, McLaren no longer feels the need to qualify her quasi-scientific fact. McLaren seems to have abandoned the explicit distinctions between the domains of ethics, science and society, and makes a case for a strong distinction in development entirely through the proxy of biological facts. The boundaries between these epistemologies seem to blur as she takes on more confidently her role as a scientist in the debates. In the CIBA chapter, she now talks only of 'bottlenecks' and 'discontinuities' in development. She no longer refers to the sociological logic underlying the distinction, but simply tells a story of increasingly restricted developmental potential.

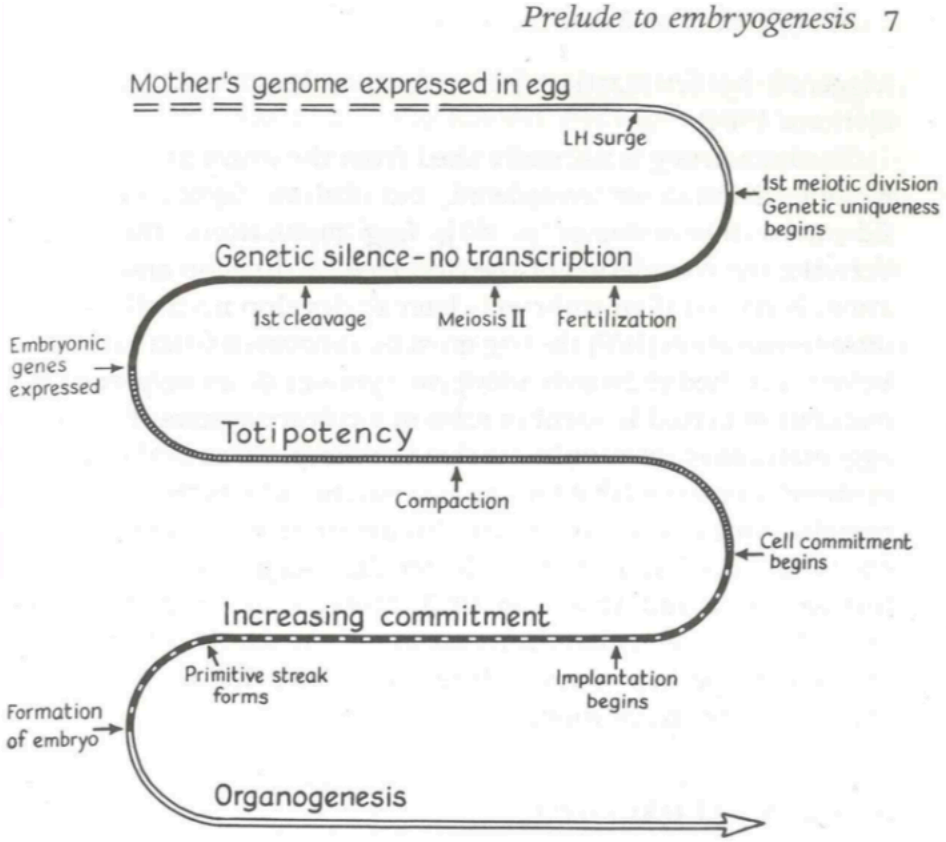


Figure 3. Linear embryonic development, "Prelude to Embryogenesis", in *Human Embryo Research: Yes or no?*, CIBA Foundation, 1986, p. 7. This linear diagram shows the restriction of cell totipotency[3] over the course of embryonic development. Like the spiral diagram in figure 1, McLaren points to several thresholds in development: genetic and physiological. In this diagram, however, McLaren omits the cyclical germ cycle and focuses solely on embryonic development to make a case, based on the physiological coherence of the embryo, for the primitive streak as a pragmatic landmark around which to build a research agenda in the area of developmental biology and reproductive medicine.

McLaren's more positivistic rhetorical approach is also reflected in a more linear depiction of development (figure 3). The diagram shows clearly a restriction of fate potential, omitting the spiral that reminds the viewer of the continuity of the germ line and the interactions between cell populations throughout the life course. It seems that McLaren has changed tack somewhat in her approach to making the 'pre-embryo' distinction, abandoning her detailed explanation of a fuller biological picture. She is now speaking in the authoritative language of scientific facts to an audience that she hopes to recruit to her model. The distinction between 'pre-embryo' and 'embryo' makes sense in light of the current state of knowledge in the field and also delineates an agenda for researchers. By introducing and defining the term at the conference, she is thus providing biological

[3] Totipotency describes the ability of a single cell to divide and produce al of the differentiated cells in an organism.

information while also inviting researchers to refer to, build upon, and exploit this scientific model. McLaren is now building a much more pragmatic case in favour of embryo research that echoes Warnock's pragmatic ethics and this is expressed in a translation of the biological vision that becomes increasingly ensconced in this pro-research case centred around clinical translation.

This history shows how the biology deemed to be 'relevant' changed as the case evolved, and this is explained by its connection to a host of other translations in the evolving pro-lobby case. McLaren thereby also takes on a role that is broader than the one she describes in *Where to Draw the Line*. In addition to providing biological information as a basis for constructive debate, she is connecting a specific period in development to an entire research agenda centred around clinical applications by connecting researchers and gathering information, as well as performing the ongoing work of calibrating these translations – the biological account changes to reflect the priority placed on clinical translation. This role as a connector, orchestrator and calibrator becomes very important to the development of PGD over the course of the debates, as we shall see in the following chapter. For now, I will go on to describe how the first two translations – one of biological facts and one of clinical applications (which, we have seen, includes professional translation) –increasingly provided common ground for scientists and potential patients in the form of a shared agenda that felt right.

Connecting and feeling

Soon after the release of the Warnock Report and the 1985 CIBA Conference, the term 'pre-embryo' was introduced into the official public information material of the pro-research lobby. The lobbying organisation, Progress, set up under the joint leadership of scientists, physicians and parliamentarians in November 1985, for example, made a systematic effort to use the term to refer to the first two weeks of development. Member and eventually chair of the organisation, Virginia Bolton, recalls,

> I think we all felt a little bit uncomfortable using it [pre-embryo], because [hesitates] it was an artificial term I suppose, but the motivation behind using it was supported and there was a great need to educate people and to be able to distinguish. But it is a blurred line, it's a continuum the whole developmental process, so it was an arbitrary term. But it wasn't motivated in any way, none of us had any motives but the highest, we just wanted to avert what would have been such a retrogressive step for the wrong reasons. It was a bit like Brexit, if you're going to have a massively important vote that is going to change the course of history then you want voters to be informed. So it was about making that vote based on rational judgement rather

than gut reaction and motivated by prejudice and misinformation. … *So we had to try to find some way of continuing to project a scientific objective attitude, and the 'pre-embryo' – we all unified and used this term all the time, we were good about that – then there was a sort of unified approach saying, "this is an entity, that hasn't decided it's fate, that hasn't yet formed, and, actually, the vast majority won't have the potential to go on and develop any further". So, it was a huge message to get across to people.*

Bolton, interview with author, 16 October 2018 [emphasis added]

Bolton reiterates the two features of the pro-research lobby case that I have described: the first that the scientific community had to find ways to translate a biological view of continuous development for the purposes of law, and that these distinctions, given biological continuity, would always have to be made according to 'non-scientific' criteria. This is the translation of a biological vision I have described. She then also shows, again, how scientists united around the use of a term and a biological account that, as we saw at the CIBA conference, represented a purposive, politically motivated, professional alliance geared towards clinical translation. This apparently purposive use of a term and an agenda, Bolton says, caused some discomfort to scientists who saw that, scientifically, the biological account they were giving was 'arbitrary'. She goes on to describe, however, how this discomfort was overcome by a greater sense of purpose, a wish 'to avert what would have been such a retrogressive step for the wrong reasons'. This logic in itself could be described as utilitarian, or pragmatic, or even Machiavellian – the ends justify the means – but it is also significant that Bolton emphasises the sincerity among scientists at the time, their genuine conviction that they were not trying to mislead, that none of them 'had any motives but the highest' but had to find a way of conveying that emotional belief to non-scientists, by finding a language that could inform people. This is how Bolton explains the narrative on which the pro-research lobby landed, as an attempt to capture both the biological view of development and the scientific conviction that research was right. This, led them to find a language that did not transport the literal meaning of their biology, but the implicit meaning, and this translation was done by identifying where the scientific account resonated with public sentiment. Bolton expresses the same challenge that McLaren tackled with her socio-biological account, namely that scientists in these debates had to choose accessibility over technical, scientific accuracy. The two were mutually exclusive. Bolton also interestingly goes on to offer a political analogy for the decision the pro-research lobby made. The consequences of a failure to translate technical accounts into a socially meaningful language result in uninformed votes in which people vote according

to “gut reaction and motivated by prejudice and misinformation”, just “like Brexit”, she says. Bolton’s recombinant reasoning offers a political rationale for the pro-lobby ‘approach’, for why scientists chose a translational application of a piece of scientific terminology. Scientists ‘unified’ behind this approach, but the approach was built on divisions, ambivalences and discomfort, resulting in a case that is, as Warnock has said, “curiously difficult to explain” (Warnock, interview with author, 21 August 2018). The cracks and divisions remained visible, but that did not undermine the efficacy of the approach – if anything, as Bolton shows, it was the ambivalence scientists felt having left their silos of scientific literalism, that made them more attuned than ever to the sentiment underlying their message, and so able to engage with the public over a shared understanding of the issue at hand.

Bolton goes on to show how scientists were able to connect with potential patients over the potential of therapeutic applications:

> So in terms of persuading potential patients, because it [IVF] was so new, it was this beacon of shining hope for people who thought they’d never be able to have families. And so the language that they understood was, “IVF doesn’t work most of the time”, which still applies today, and “embryos are generated that don’t have the potential to develop and some of them do, and we want to be able to distinguish the ones that do from the ones that don’t”. And in order to improve the success rates, we need to understand what the embryos need in order to grow and develop. All of this is in such a rudimentary stage, there’s so much we don’t understand, and if we are to help all these people who want to help their families, we have to …
>
> Bolton, interview with author, 16 October 2018

Bolton shows how the so-called ‘language’ used by the pro-lobby provided an emotional connection between potential patients and scientists. Clinical applications represented powerful examples of the benefits of scientific research on human embryos and in this sense translated the embryologists’ conviction that research was moral and felt right to non-scientists. As in McLaren’s plan for the CIBA conference above, Bolton’s comments show how clinical translation became a key part of the pro-lobby strategy, of developing a consistent scientific case, and of recruiting a larger group to a scientific agenda. Bolton’s statements also add, however, how this scientific rationale relied on sentiment in order to guide them in finding an appropriate translation of the scientific facts. The Warnock model became part of an attempt to find emotional common ground through a shared language of clinical translation. This was a bilateral process of translating public concerns into a language that could be considered authoritative and scientific, and of translating science

into something that was considered sympathetic and relatable. The above account shows how the biological facts in the evolving case for research represented much more than a shifted ontological trajectory, as has been argued in previous analyses of the term (Jasanoff, 2005; Mulkay, 1997), but were part of a renegotiation of the very framing of the ethical question at hand, and how this question went from one concerning the status of the biological entity to a social question of the good that science could do for society in the form of clinical applications. Having understood the framing and evolution of the pro-lobby case, I will now move on more specifically to how the use of the term 'pre-embryo' indexed a changing role for scientists in the policy debates.

The 'pre-embryo'

The term 'pre-embryo' was first officially introduced into the pro-research lobby rhetoric by the Voluntary Licensing Authority (Franklin and Roberts, 2006; Mulkay, 1997). It was after they introduced the term that it spread, as if by "osmosis", in Bolton's words, to other organisations (Bolton, interview with author, 16 October 2018). Here, too, McLaren was a major player, participating in the series of meetings of the joint Medical research Council and the Royal College of Obstetricians and Gynaecologists Voluntary Licensing Authority, chaired by Dame Mary Donaldson. Again, as the most senior practicing embryologist, McLaren was able to direct the Committee on the scientific basis for their discussions. The minutes of these meetings clearly document how the term 'pre-embryo' became officially endorsed as the correct terminology by the VLA. The term was first used in a pamphlet produced in May 1985, which the committee had asked one of the Committee's 'lay' members, Dr Penelope Leach, to write at the first meeting on 26 March 1985. The pamphlet was intended to provide "a statement on *in vitro* fertilisation that might be suitable for the general public'" (RCOG, C12/1). The term was then approved by the Committee at the second meeting held on 16 May 1985. The minutes to this meeting state that

> Members were initially divided as to the use of the term 'pre-embryo' introduced in Leach's paper. Some members consider that 'embryo' was too emotive a word to be used in the context of Guidelines on research and in other published documents, others suggested that to replace 'embryo', which the VLA has used freely to date, would be regarded with suspicion by the press and public. Members discussed the use of alternatives to 'embryo' and it was decided that 'pre-embryo' should, whenever possible, be used instead.
>
> Minutes MRC and VLA Committee meeting, 16 May 1985, RCOG, C12/1

Dr Leach was a social psychologist, journalist, broadcaster and author, and thus

well-suited to the task of delineating and communicating development to a broader audience. Attached to the draft of the pamphlet sent to the committee, Leach outlines the key considerations kept in mind when writing the text, and also acknowledges that "Dr Anne McLaren has been most generous with information and in giving her time to reading an earlier draft" (ibid).

In a phone-interview with Leach, she confirmed to me that she collaborated with closely with McLaren on the pamphlet, that McLaren provided the scientific information and she then wrote up the information on which they agreed. The term was needed, as she recalls, to "separate the fertilised ovum from the embryo" and when Leach reasoned, "well, it's pre-, it's a pre-embryo … Anne said, 'that's a good idea" (Leach, interview with author, 1 October 2018). Leach emphasised that the term itself did not really seem to matter to McLaren, and that she was amenable to adopting terminology that made sense to the non-scientist, for which she relied on Leach's judgement. Leach's collaboration with McLaren, then, helps showcase how McLaren used her position as a scientist who held information that was essential to the debates, and combined this with a commitment to clarity that would only be achieved by finding terminology that foregrounded the distinctions that were decisive to a broader constituency – through non-literal translations.

McLaren clearly directed the scientific narrative used by the VLA – indeed, Leach recalls how the "strange constitution of the VLA", where members were selected based on their relative ignorance of the embryological facts so that that the group would reflect the attitudes of a pluralistic public, actually gave an eminent scientist like McLaren a lot of influence as one of the few in possession of any scientific information, and with a lot of authority and experience to make this perspective heard (Leach, interview with author, 1 October 2018). As on the Warnock committee, McLaren's influence as the only scientist gave her control not only over the scientific narrative, but the utter reliance on her for what came to be seem as a scientific question, also meant that she was perceived increasingly as an authority. The amount of sway McLaren was able to have, then, points to the authority that science held as a domain of knowledge in the debates. We begin to see that the promise of scientific discovery as a narrative of hope that was so essential to the pro-lobby case (see Mulkay, 1997), relied on science as a privileged domain of expertise and, moreover, substantiated a particular type of expertise that combined ethics, feeling, and legal insight, through the rhetoric of science, embodied by public intellectuals like Warnock and McLaren in the debates, a point to which I return at the end of this chapter.

Leach recalls, then, that the crucial concern to McLaren, rather than the precise terminology used, was simply that "a distinction was made" (ibid). McLaren, as in *Where to Draw the Line*, remained clear about the need for limits, but the term used to demarcate these was arbitrary; it was the underlying rationale that was important. Indeed, a closer look at the pamphlet Leach and McLaren produced shows how she used the opportunity to make a stronger case for this distinction in development, now connecting it to its utility on the path to clinical translation.

The pamphlet opens by rehearsing the history of the setting up of the Warnock Inquiry following the birth of Louise Brown. Leach then refers to the statements of the RCOG and the MRC following the publication of the Report as evidence of a consensus among "the medical and scientific professionals concerned with *in vitro* fertilisation" about "how the work should be handled and controlled", but that it also became evident that "both the facts and the implications of the work remained obscure and – consequently – alarming to many other people" (Leach, 1985, RCOG C12/1, p. 1). In light of this fear stemming, supposedly, from ignorance, the pamphlet seeks to inform the public primarily on IVF – on what it is, its "purposes" and "implications", and the "controls" being exercised (ibid.).

Unlike in the CIBA symposium structure or in *Where to Draw the Line*, the biological account of embryonic development here does not introduce a discussion of possible applications but is presented from the beginning in the context of an artificial intervention, as opposed to the natural biological cycle. Leach next describes the use of IVF and embryo transfer as a way to bypass blocked fallopian tubes and how this work was only possible as a result of research on human embryos. She goes on to compare public views on research on gametes to those on embryo research. Resistance to the latter is based on a false conception of fertilisation as the start of individual life, she claims. She explains that fertilisation is itself not sufficient to "start a human baby but merely sets in train a series of events which could lead to such a beginning. Fertilisation is a necessary but not sufficient condition" (Leach, p. 3). She goes on to introduce the now familiar narrative of progressive cellular specialisation following the establishment of genetic uniqueness at fertilisation. She also introduces into this narrative the possibility of intervention to combat the natural inefficiency of the process. Cell division following fertilisation, "whether in a fallopian tube or a glass container", for example, can be induced by "electrical or chemical stimuli" (ibid, p.4.). These references to possible artificial assistance to biology, whether IVF and transfer, or induced cell-division, Leach writes, are not intended to incite images

of a futuristic biology, but to show that biological potential is always relative to its environment:

> The point is not that human pathogenesis is around the corner but that this egg is not yet a potential baby but merely dividing cells … at the eight-cell stage, when the egg reaches the uterus, there is no way in which cells which might be destined to become a foetus differ from those which might be destined to become part of a foetus' support and attachment system: amnion, yolk-sac or placenta.
>
> Leach, 1985, RCOG, C12/11

This sentence, then, expresses the crux of the pamphlet, which resonates given the framing of the piece, namely that biological development is contingent on a receptive environment, and this environment can be both 'artificial' or 'natural' – in either case, the embryo itself is never sufficient to express any type of potential. It is at this point that Leach introduces the term 'pre-embryo' to name the "cluster of undifferentiated cells" (ibid.). She then goes on to describe how the 'pre-embryo' might implant in the uterus "if conditions in the womb are exactly right", and if they are not, how the "pre-embryo will simply be passed in the menstrual flow" which is the case for over "half of all pre-embryos" or, more whimsically put, "the shedding of fertilised eggs, unrecognised and unmourned, is an everyday occurrence in the unending cycle of human fertility" (ibid.). Now Leach returns to the individuality argument, that it is not until the formation of the primitive streak that it is "possible to speak with any scientific accuracy of individuality or potential individuality in the product of fertilisation" as it is only now that we can say that "if there is one primitive streak there will be (if all goes well) one foetus" and if "there are two or the primitive streaks there will be (if all goes well) twins or triplets" and so "now, and only now, has the pre-embryo developed into an embryo" (ibid.). The embryo is semantically sequestered from the entity that might or might not lead to a child. It is only now that we return definitively to the body, and "only now will a normally-fertile woman begin to realise that her period is late and that conception has probably taken place" (ibid).

The narrative that until now ambivalently hovered between the biological and laboratory environment, now returns to the woman's body, to indicate the onset of a new phase in development and an entity that can be described using the conventional teleology of 'embryo to child'. This shift back to the body shows how the 'pre-embryo' relies on a dual narrative – it is simultaneously treated as part of the trajectory that might lead to a baby, while it is also treated as a researchable entity that in no way inevitably leads to a child and can, therefore, be manipulated without moral qualms. This dual structure is the

result of a developmental story that depicts the 'pre-embryo' as a set of potentials that are expressed differently according to its environment – as assisted.

The clinical interventions in this cycle make this contingency, this 'biological relativity', as Sarah Franklin (2013a) has described it, demonstrable. As a result, IVF embryos have remained what Jasanoff and Metzler (2018) call "bioconstitutionally ambiguous", as "neither inanimate research materials nor full-blown human subjects, but entitled to constant moral revisitation as science progresses and new potential uses, and perceived abuses, come into view" (2018, p. 10). The 'pre-embryo' is defined in relation to a consensus application, and thus its status changes in accordance with new applications.

The Warnock model facilitated this legislative format and it is consolidated in this pamphlet by drawing on a clinical framing. The clinical framing becomes coextensive with an effort to persuade the readers of the pamphlet that the 'pre-embryo' is distinct from the embryo-child, by showing how the early stages of development can be variously facilitated by offering more amenable environments in the form of artificial interventions. As we shall see in the following chapter, the case of PGD builds on this new biological relativity captured in the Warnock model, so that talking about basic science increasingly also becomes a case of encouraging clinical applications as a proof of concept to validate a biological image of contingent development in the public eye. The implications of this change in public perception of development has become evident post-HF&E Act. The notable lack of opposition to amendments to the Act since 1990 to incorporate new types of experiments, for example, suggests widespread acceptance in Britain of a new model of development allowing a new legislative model that responds to emerging clinical possibilities (Franklin, 2013a; 2014b).

The public scientist as clinician

This new biological picture also prescribes a different role for the scientist. McLaren's move into the more public debates on human embryo research is coupled with her claiming authority on clinical translation. In responding to the perceived concerns of a broader constituency, McLaren has instructed Leach on the basic embryological facts but only in the context of clinical application. As in the CIBA correspondence, McLaren is moving into a public role that requires her to comment on both basic science and clinical translation. As Warnock said, when asked how McLaren perceived her role in the debates,

I think it changed, in the sense that she realised much more acutely how actually

> the gap between pure and applied science was flexible. And that she couldn't simply think of herself as a pure research scientist without taking on board the fact that the application was always going to be hovering there, and inseparable in a way from the pure research. I think that came to her during the life of the committee and thereafter.
>
> Warnock, interview with author, 21 August 2018

McLaren increasingly takes up a role that blurs the boundaries between basic and applied science or, at least, brings them together to show how they relate, in a sense recapitulating the clinical translation of IVF in her professional public remit. Warnock also highlights McLaren's growing sense that commenting on application was her social responsibility as scientist – application was 'inseparable' from 'pure research', a fact that could not, in all sincerity, be ignored. Again, this points to the way in which clinical application was a reiteration of scientists' feeling that their work was morally right. In part, clinical translation made this conviction tangible for an audience of non-scientists, but in doing so it seems that, in McLaren's case at least, this led her to see clinical application as an increasingly important aspect of her scientific work – as more than a rhetorical strategy, but also a social responsibility that should in itself be part of the work of doing science. This becomes increasingly clear in the following chapter, where I describe her contributions to the development of PGD during the debates. For now, it suffices to comment that clinical translation fell increasingly under her domain of expertise in her role as a public scientist.

Superimposition: A translational model

If, then, McLaren's basic science became more applied, a view that resulted from an attempt to build an allegiance based on a moral sense around a scientific model, the public perception of basic biology also changed. The effectiveness of the Warnock model in achieving consensus by the end of the debates shows that non-scientists had been recruited to a new, relative view of embryogenesis (Franklin, 2013a). This was achieved by packaging a description of development in a story about clinical translation. This story, as we have seen, nonetheless kept intact the story of the embryo-child by showing what research could do for potential parents; the biologist's vision and the popular vision are brought together into a single account that respects public sentiments and shows how they might relate to those of the biologist.

This results in a model of development in which two embryos coexist: one teleological and one contingent. This biological picture was different from both the

biologist's and the lay perspective as it incorporated both. The case in favour of research post-Warnock could accordingly be described as a superimposition of these views. Both views, we have seen, were based on emotional conviction, and what Bolton calls the 'language' of clinical translation that maps the intersections of these two, moral world-views. The rhetorical strategy is in this way about describing the relations between overlaid world-views, of connecting, and thus leads us to another sense in which translation was used to arrive at the final HF&E legislation – in the geometrical sense, as the "movement of a body from one point of space to another such that every point of the body moves in the same direction and over the same distance, without any rotation, reflection, or change in size" (Oxford Dictionary Online).

Here, bodies of knowledge, of feeling, science, law, and now clinical application, are all translated in accordance with a practical question of how to use the human embryo, in order to find points of convergence. The picture that emerges is changed for all those concerned, and that is only right, considering that it expresses a new consensus, a shared biological model. This leads to a model for policy-making that might be drawn as in figure 4. McLaren, following Warnock, superimposed a scientifically based vision of human reproduction onto a regulatory provision, and now clinical translation, to form an overdetermined area of convergence that provided a foundation for the Government in their response to biotechnological innovation. It was, in this sense, what Sheila Jasanoff (2005) calls a 'settlement' (see also, Jasanoff and Metzler, 2018) – the term she uses to describe how policy on biotechnology is made and how these frameworks provide the context for future expressions of opinion. As the geological idiom implies, it is a framing of the problem through a compression of layers onto each other to build a more secure footing on which to base policy.

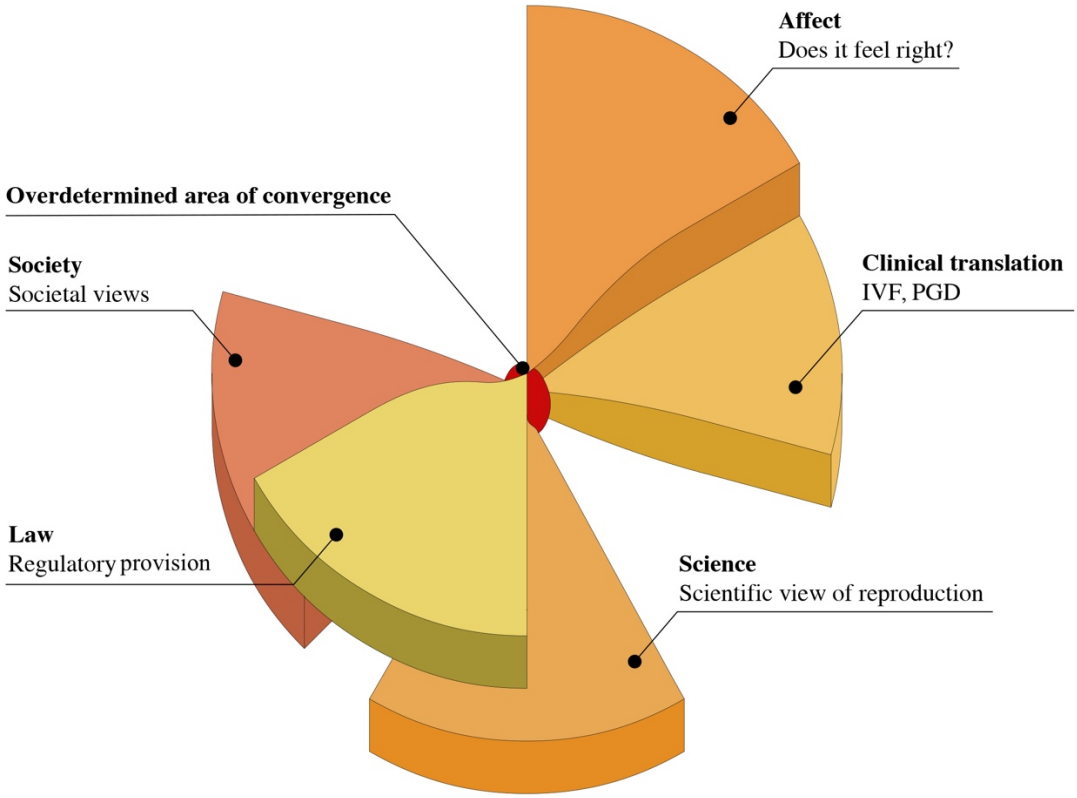


Figure 4. A model for policy-making based on superimposition.

In figure 4 I have schematised the various 'logics' that Anne McLaren uses in her developing case in favour of human embryo research over the course of the debates. These epistemological perspectives are brought together to form an overdetermined area of convergence onto which legislation could then be built. While the various tectonic plates never fully overlap, this common ground allowed the pro-lobby to unite behind a shared research agenda and, increasingly, a shared socio-biological perspective that allowed legislation to be passed.

Having described some of McLaren's specific contributions to the development of the pro-research lobby case, I will now go on to further characterise the argument used by the pro-research lobby, and the role that arguments for clinical translation played, by referring to my interviews with other scientists who were active in the pro-research lobby. These interviews help provide a context for understanding McLaren's choices as she negotiated her position in the debate, and also contribute to an understanding of how the pro-lobby arguments worked to position scientists in the public debates more generally.

Scientific contestations

In my interviews with the scientists who lobbied during the debates, two distinguishable

components of the pro-research case are repeatedly emphasised as key to their parliamentary victory. The first relates to the translation we have seen above: of the biological facts into something socially palatable; and the second is the role of clinical translation. The picture they paint, however, differs from the one I have described vis-à-vis Warnock and McLaren. The first translation is, for one, not presented as a negotiation of the biological facts in relation to the society, but as a unidirectional imparting of objective scientific facts. Moreover, the relationship between the biological narrative and the case for clinical translations is also disregarded – most scientists referred to the public mass-education on the embryological facts of development as an important first step, and the prospect of clinical applications resulting from research as an additional and more decisive factor in the pro-lobby case. The scientists who were most senior in their careers at the time of the debates repeatedly claimed that the public readily accepted the scientific narrative used by the pro-lobby, deeming the use of a so-called 'artificial' term such as 'pre-embryo' unnecessary.

There is, then, a tendency in my interviews with senior scientists who worked on the pro-research lobby to erase the evidence of the very translational process that they themselves invented. This translational amnesia is reflected in the scientific literalism of the statements made by the scientists I interviewed, exemplified by comments made by the embryologist and geneticist Richard Gardner, who was at the University of Oxford at the time of the debates, as well as a member of the Royal Society (Gardner and Graham, interview with author, 27 September 2018). Gardner acknowledges the importance of the underlying rationale for the 'pre-embryo', but sees the term itself as a 'fudge':

> Well, I think both Chris [Graham, fellow embryologist] and I were more on the side of trying to explain the status of these earlier stages, the biological status. Because that was one of the falling issues, that we had all been in terms of terminology, very lazy. Because we'd all referred to everything from the two-cell stage onwards as an embryo and…it's an embryo in the case of a frog because all of it is going to go on to form an individual, but only a fraction of it is in mammals. And so we suddenly realised when we were talking about doing research on early human embryos. And people became exorcised as to what to do, and I think Anne was among those who was responsible for a fudge that actually didn't help because it engendered suspicion, which was to talk about this as a 'pro-embryo' or a 'pre-embryo'. Whereas if we'd had our wits about us, it would have probably obscured people, but it would have been more accurate, if we'd talked about the conceptus, the whole of the product of conception…only a fraction goes on to form the individual. But actually, in that context the 'pre-embryo' is very unhelpful because it was regarded by the public, quite rightly, as scientists trying to disguise what they were trying to do – terrible things to little hominids.

Gardner and Graham, interview with author, 27 September 2018

Gardner here acknowledges the importance of a terminology that would reflect accurately scientists' sense of the biological material they were working on, but he sees the term 'pre-embryo' as a failed rhetorical instrument that misled the public. Nonetheless, he says that the distinction it denotes is scientifically accurate, based on a retrospective relationship to embryonic and extra-embryonic material in human development. He later refers to the argument used to substantiate the 'pre-embryo' divide as one that counteracted the damage done by the introduction of the term 'pre-embryo' itself:

> I think you have to engender public confidence and by sort of making the argument- rightly or wrongly – that at 14 days and the primitive streak you have the first point at which a nascent human being started to develop I think gave a lot of reassurance to people who may have been disquieted by the idea of the 'pre-embryo'.
>
> Gardner and Graham, interview with author, 27 September 2018

Gardner moreover sees the role of clinical translation, rather than the biological arguments used, as decisive and separate from the biological case made, saying "I think once people accepted IVF, they acknowledged there would be spare embryos, and that would have to be part of the package, and then they accepted it" (ibid). This idea was reflected across many of my interviews. Clinical geneticist, Marcus Pembrey, head of the Mothercare Unit of Paediatric Genetics at the Institute of Child Health in London and an honorary consultant clinical geneticist at the Great Ormond Hospital for Children, as well as a Progress campaign committee member and chair, similarly argued in his interview with me, that, rather than the biological mode used, the more crucial factors in gaining public support for human embryo research, he claims, was that "genetics came along at the right time and then pre-implantation diagnosis came at the right time, and that's what persuaded the progression of the science leading up to it" (Pembrey, interview with author, 17 October, 2018).

There seems to be ambivalence around the relationship between the 'pre-embryo' and the biological facts used to inform people in the debates, as well as that between the arguments for a distinction in development and those about clinical translation, a trend that also appears in the sociological literature on the debates (see Mulkay, 1997). As opposed to the model we have seen, described in the comments by Bolton and enacted in the methods of McLaren, where the 'pre-embryo' served as a rallying point that allowed scientists to determine and then explain how the early stages of development could be

manipulated in aid of clinical therapies, these analyses position the biological facts and the clinical application as distinct. In the same way, Mulkay (1994) argues, in his analysis of the 'pre-embryo', that the links made in the pro-lobby argument to PGD were "decisive" , and built upon the arguments made for the 'pre-embryo' as a separate ontological entity, arguing that while "for the majority, the replacement of the embryo the pre-embryo made further research seem permissible", the "displacement of the violated experimental subject by the genetically screened pre-embryo made its continuation appear obligatory" (1994, p. 633). While genetic screening, as we will see in the following chapter, undoubtedly provided a shared goal for the public and scientists, Mulkay's argument that this built on the 'pre-embryo' as a distinct argument overlooks how this clinical aim was negotiated.

Interestingly, the scientists I interviewed who were in the early stages of their careers at the time of the debates, and most intimately involved in the public lobbying efforts, see the use of the term 'pre-embryo' differently and discuss the role of the 'pre-embryo' in relation to the role that biological facts and clinical translation played in what is described as an 'approach'. Experimental embryologist, David Whittingham, who had set up the Mammalian Development unit at University College London with McLaren in 1974, talked about this difficulty:

> The media thought of foetus with an embryo, thought of it as something recognisable. When you're trying to explain that the cells are undifferentiated but have the potential to develop into an individual – it's a concept that's very difficult. Even today people don't see that two or eight cells are an embryo, in their mind they visualize something with limbs and a head. So the argument for going as far as 14 days, was that up until then you don't have a body plan. When you have the primitive streak, you have a foundation of the embryo or foetus. It's a difficult one to argue when people feel that with fertilisation as such – once you have two cells – you have an individual with a soul. What happens when you get identical twins, do you cut the soul in two? [laughs] I'm just joking. … Those were difficult arguments with people who believed in life from conception – not 'potentially' – but the start of a new individual.
>
> Whittingham, interview with author, 3 October 2018

Whittingham sees the primitive streak argument as, in part, an important visual signifier, based on a progressive account of development, that could counteract the image, so prominent in the media, of the embryo as a foetus. He also explains, however, that the argument was fundamentally limited in its persuasive power because those who believed that life started at conception were never going to get on board with this biological picture. Whittingham begins to describe how the term represented an alliance based on more than

biological facts, but also a basic agreement on the question being asked – namely, how the embryo should be used, which assumes, from the onset, that there are cases in which it is morally permissible to use it for purposes other than reproduction. Virginia Bolton, who was an early career embryologist at the time of the debates, elaborated on this alliance:

> So she [McLaren] could then alert us to the sorts of things that people feared, fears that we had to obey but also sort of – there were two camps in terms of the opposition. There were the fundamentalists, for whom nothing was going to change, they had to be dealt with, and she probably taught us this, but it became instinctive after a while, that fundamentalists you must never engage and start an altercation, you have to respect their views and back off because that is how they feel, and you have to respect their opinions no matter how misguided they might be. And you always had to remain dignified and detached, because there was nothing to gain from engaging. ... *But the one that we engaged with were the ones who didn't understand, who were fearful and had prejudices based on a lack of understanding. And that was it, it was about informing people. "Do you realize this is what has led to this situation? This amount of research has gone on and this has allowed what we have here. And had that research been banned, none of this would have been possible. And do you see what its potential? Reducing risk, improving IVF, reducing genetic disease."*
>
> Bolton, interview with author, 16 October 2018 [emphasis added]

Bolton reiterates the point made by Whittingham, that the biological view being communicated precluded the support of fundamentalists because it contained certain assumptions that were irreconcilable with their position – namely, that it is in principle acceptable to use human embryos for a purpose other than producing a human baby. As we have seen, this assumption framed the entire remit of the Warnock Report, which gave answers to the question of how the human embryo should be treated, rather than its moral status. Bolton shows how this question translated into a pro-research campaign – under the banner of research in aid of clinical translations. In this sense, scientists were 'informing' a wider constituency that was in principle open to the use of human embryos for research, but required a moral justification, offered to them in the form of a scientific narrative that emphasised the ends towards which the scientists' investigations into early embryonic development could be channelled. In contrast to the assertion made by some scientists that the biological picture served in the debates to offer an alternative to the image of the embryo as foetus, Bolton and Whittingham show some recognition of how the pro-lobby's scientific arguments contained not just facts, but an implicit framing geared towards a specific audience.

Bolton's comment also shows the extent to which the arguments for the 'pre-

embryo' and its applications prescribed a very particular role for embryologists in the debate. Given an implied target audience of those in principle receptive to scientific arguments, scientists would not have to speak to basic ethical principles, at least not overtly, but about biological facts and, increasingly, clinical applications. Once a target audience had been identified, the role of the scientist would be to describe a scientific agenda under which scientists and the under-informed-but-willing could unite. This explains why scientists reflecting on the arguments used might refer only to the scientific facts used, especially clinical translation, and not the underlying rationale – the model of deliberation over which scientists united facilitated this and positioned them as such. As in the account I have given of McLaren's shifting position, these scientists refer to clinical translation as a proxy for the underlying ethical, legal and social assumptions of the pro-research case. These clinical translations substantiated the biological picture they already believed to be true and recruited the general public to this view.

While interviewees such as Virginia Bolton, who was an active member of Progress and was thus more involved in the public engagement side of the debate than the scientists who chaired committees or briefed politicians, were able to explain how the 'pre-embryo' represented not just a set of biological arguments but a "unified approach" (Bolton interview with author, 16 October 2018), the comments by scientists who were more senior in the 1980s, like Gardner, seem to perform a process of 'papering over the cracks' of the superimpositions that were required in the translational process required to arrive at legislation. These scientists refer to a model for the role of science in public debate in which the scientific facts form the starting point, which are in turn imposed on the debate by scientists in order to arrive at legislation. Yet this model performs exactly the division that the consensus is supposed to 'paper over'. Preferable would be a theory of translation applied to the biological facts that does not suppress fracture, that, like the Derrida's poststructuralist theory I have described, positions difference as the source of meaning, that allows the various layers of the palimpsest to show through in order to render the scientific facts for a specific audience. The account of McLaren's developing pro-research case reveals how scientific literalism is neither more ethical, nor more correct, but ignores the work that needs to be done in order to arrive at a shared understanding of the values, possibilities and social priorities that come with any translation of the biological facts.

While on one level the comments by scientists may reflect a professional claim to the space and a need to sequester themselves from political involvement, there is also

undeniably a persistent confusion and difficulty that characterises attempts to explain this unprecedented process of policy-making in the area of human fertilisation and embryology. The development in the arguments used by McLaren, who consistently participated in and even orchestrated the debates at various levels, provides a useful index for these changing attitudes, and also helps explain the discrepancies in the perceived legacy of the arguments by scientists. As we have seen, McLaren takes an increasingly positivistic and authoritative approach to communicating the embryological 'facts' reflected in a more consistent use of the term 'pre-embryo'. This is in part possible because a research agenda and set of clinical applications are increasingly used to substantiate the existence of the 'pre-embryo' as a researchable entity.

The social and legal logics underlying the distinction fade increasingly into the background as the scientist is able to comment on the safer, more 'scientific' uses of the embryo towards clinical applications, which now serves as a proxy for social and ethical concerns. By describing how this 'non-scientific' fact came to be endorsed, we have nonetheless seen how the 'pre-embryo' can be understood as part of the pro-lobby's developing case, how it responded to social attitudes, how it was part of a process of professional agenda-setting and of finding common ground. In this light, the term appears less as a deceptive 'fudge', less as political 'rhetoric', but as part of an ethical, compassionate model for policy-making in the area of human fertilisation and embryology.

Arbitrary term, arbitrary scientist?

It is also pertinent to mention at this point, the parallel that arises between the language used by scientists to describe the use of the 'pre-embryo' in the debates, and the way in which they describe McLaren's role. The language of arbitrariness used by scientists to cast the 'pre-embryo' as unscientific and to distance themselves from this use of 'rhetoric', is also reflected in the way that they discuss McLaren's career. The fact that she became more involved in policy work from the Warnock Committee onwards, for example, led some of my interviewees to say that from the 1980s she was 'scientifically past her prime' (Goodfellow, interview with author, 21 September 2018). Interviewees also repeatedly drew a comparison between her and Mary Lyon. The latter was a real 'Nobel' scientist, whereas McLaren was a great 'absorber' and a 'connector' (Gardner, interview with author, 27 September 2018), a distinction is drawn between the social work of collaborating and doing policy work, and the work of basic science. It is difficult, apparently, to do both. Yet

McLaren did do both, and perhaps as a result of this connecting, absorbing, collaborative approach, as we will see, was also able to have a greater scientific impact. In the context of the Warnock debates, too, McLaren combined the logics of science, law, ethics, and feeling, in order to make a case for research.

This led scientists like Robert Winston, who was involved, as we shall see, in the development of PGD, to use the ambivalent language of 'emotion' to describe McLaren in his interview with me. He calls her an "academic scientist" with "a humanity about her", as someone who was "feeling her way through things" (Winston, interview with author, 26 September 2018). There seems to be a tension at the heart of McLaren's legacy as it is described by scientists, that makes her difficult to place. What makes her distinctive, it seems, is the way in which she draws together science with extra-scientific domains – domains often associated with human 'values' rather than facts.

The fact-value distinction has been the subject of critique in a far-ranging literature in Science and Technology Studies (STS). These interventions have been framed around the outdatedness of a dichotomy in which values are associated with the public and facts with the domain of authoritative science. Bruno Latour and Steve Woolgar in the classic Laboratory Life (1979), for example, challenge the distinction while developing an ethnography of modern divides, as does Latour in *We Have Never Been Modern* (1993). These anthropologies of science show how the 'two cultures' of society and science are actually one – that facts are socially constituted, values are inherent to the process of arriving at the supposedly value-neutral knowledge that masks the conditions of its production.

The study of translation from this perspective also makes an important addition to this body of work in literature by emphasising the context of the 'postgenomic' era of biomedicine. (e.g. Webster 2005). The anthropologist Kaushik Sunder Rajan and sociologist Sabina Leonelli (2013), for example, use the concept of translation in the 'post-genomic' era as a site for challenging the very truth/value categorisation. "Translational research", they argue, "is a critical site at which knowledge/value problematics emerge and can be theorised" (Rajan and Leonelli, 2013, p. 465). The breadth of domains and fields incorporated in the movements of elements that constitute biomedicine opens up a process, they argue, by which a plurality of values become visible: "the monetisable/fungible/tradable kinds of value that speak to the construction of a stronger commercial infrastructure for the life sciences", the "accountability value of bureaucratic

audit cultures", and the "ethical value enshrined in projects that emphasise the "advancement of human health" as their goal" (2013, p. 465). These, in turn, are related to factual knowledge in new ways in translational research, forcing us to ask, "What contemporary biomedical 'knowledge' might mean, and to whom, given the variety of domains and circumstances under which it is produced, circulated, and used" (ibid). Translational research, then, reveals a set of movements and processes that allow the critic of science to describe how the categories of value and knowledge are and might be constituted in the biomedicine today.

McLaren's case similarly reveals how facts and values come to be associated in the process of translation. As we have seen from Bolton's comments, values played a large part in shaping the responses of pro-research lobby scientists during the HF&E debates. A description of McLaren's role in developing the Warnock model on the pro-research lobby details this process of the iterative incorporation of various facts and values. The value of clinical translation, what it could mean for human society, the role that science should play in the betterment of human health, her own sense of what mattered to people, all factored into the case McLaren made, and came to be reflected in the developmental model of the 'pre-embryo'.

It was this process of finding common ground, through science, the way in which she combined social feeling and scientific factual knowledge – or a commitment to scientific research for its own sake and a commitment to the social good it can do – that made McLaren's contribution to the debates so distinctive. This only becomes clearer as I go on to describe her contributions to PGD. McLaren provides a case in point against a definitive distinction between fact and value when describing the role of scientists in public debate. She brings these together in a translational ethos that seeks common ground, builds consensus, in order encourage scientific research that is directed towards human needs, but allows a broad enough research remit for scientists to do their work, a model which I will describe more fully in the chapter on her style of practice in Chapter 3. Here it is already clear how values and facts were key to her argument, how she consulted people and catered to them using scientific arguments.

We can also note how this combination is effective in arriving at legislation that is workable, representative and lasting, yet how this also comes to sit uncomfortably in her legacy as a scientist, an observation I will argue that reflects, as does the legacy of arbitrariness around the 'pre-embryo', a certain forgetfulness among the scientific

community regarding the translational model that they themselves helped invent.

Anti-expert experts: Science and expertise in the British debate

Through the readings of McLarens texts and an analysis of her actions and correspondence, it becomes clear that McLaren set herself up increasingly narrowly as a scientist, commenting on scientific matters as the debates progressed, abandoning the explication of the social and legal rationale for her ethical cut-off points in the context of the CIBA Foundation symposium and in the public information material provided by the VLA. The more strictly scientific case was characterised by an increased emphasis on clinical applications and a more positivistic assertion of the 'pre-embryo' as a scientifically distinct phase in development. It becomes clear, especially after reading this story in dialogue with the comments made by other lobbying scientists, how the scientific case in favour of embryo research was in part also a negotiation of the role of scientists in the public debates. The increasing emphasis on clinical applications and the pre-embryo become proxies for a set of superimposed perspectives that inform the distinctions made. On one level, this transition reflects an increasingly forceful and unified pro-lobby case. Yet McLaren's role also needs to be seen in the context of Britain's relation to expertise.

Historian of science, technology and medicine, Duncan Wilson (2014), has explained Warnock's influence and success in the history of what he calls 'British bioethics', distinct from the American history of the field, by showing how her personal philosophy resonated with the Conservative emphasis on individual consumer choice and anti-expertise (see also Chadwick and Wilson, 2018). Wilson describes the unusual blend of philosophy, pragmatism and affect in her reasoning for the Warnock recommendations partly as a negotiation of her role in light of a general distaste for expertise and a mood that favoured external regulation and external involvement in scientific and medical ethics under Thatcher. Warnock clearly demarcated a role for herself distinctly as a philosopher, in which her remit was to clarify the properties and consequences of a particular moral standpoint, but in which she could not "'prove or otherwise show conclusively that one view is to be preferred to another'" (Warnock cited in Wilson, p. 169). Philosophy was just one analytical tool, to be used alongside scientific expertise and an understanding of people's ethical feelings.

As Wilson points out, unlike the bioethicists in the United States, who were regarded as ethical experts, Warnock made a point of positioning herself as a lay person –

both in relation to science, but also to morality. Wilson goes on to argue that Warnock's notion that sentiment cannot be divorced from morality led her to espouse a limited role for the philosopher, as a moral position based on feeling was as valid as any other, claiming that "there is no such thing as authority. There is only a set of different opinions" (Warnock cited in Wilson, p. 141). This view, he goes on, "dovetailed with the neo-liberal emphasis on individual autonomy and echoed Margaret Thatcher's belief that 'choice is the essence of ethics'" and thereby became part of a Conservative effort to break the welfare state form of government that believed professional expertise was vital to the development of public policy.

The Conservatives instead argued that policy should be shaped by 'outsiders' who functioned as proxies for different stakeholders and consumer interests. Warnock echoed this belief by arguing that declining trust in professional expertise transformed what were once "matters of professional behaviour" into "questions of public policy, which merit public discussion and therefore, because we are a democratic society, ultimate discussion in Parliament" (Warnock cited in Wilson, p. 156). Warnock can therefore be seen to be setting herself up in response to a political climate that would not accept moral expertise. Instead she hands the decision to parliament, claiming only a role as an adviser, thereby contributing a limited domain of understanding in order to facilitate a deliberative and democratic process of decision-making that will be based on multiple perspectives, as well as multiple epistemologies.

Wilson's account of Warnock's self-fashioning as a public non-expert resonates in some ways with the account I have given of McLaren's evolving stance. McLaren, as we have seen, was similarly careful to distance herself from the role as an ethical authority. Her statement in *Where to Draw the Line* (1984), in which she is careful not to draw ethical conclusions based on her scientific-social account of development and the ethical conclusions that follow, but also her consolidation of an argument for research based on the scientific 'facts' of embryogenesis and clinical translation, both point to the ways in which McLaren publicly defined her contributions as, in a similar vein to the scientists I interviewed, one of explaining potential applications without offering any definitive expert judgement.

I have also shown how this account was premised on a specific framing of the question and a layering of rationales, a conversion that McLaren orchestrated. McLaren set the agenda and brought together the expertise and recruited the support needed to build a

clinical case around her model of development. The scientific case was geared very much towards finding a consensus position that incorporated the views of a wider constituency in which McLaren as an individual scientist set herself up as a facilitator, and someone with a limited domain of knowledge that could be used to inform the debates but whose judgement was not decisive.

Nonetheless, McLaren's case shows how Wilson's emphasis on anti-expertise as a unifying concept between Warnock and Thatcher's government is misguided in two important ways. First of all, McLaren's capacity as a scientist allowed her considerable authority and influence on the Warnock Committee, the VLA, and generally as she organised conferences with CIBA, for example. Ironically, it was probably the shift towards anti-expertise in the Government's approach to the forming of committees that gave McLaren such a prominent role in the first place. The committees that were formed over the course of the debates were all structured in order to be representative of a pluralistic society, seeking "individuals who had no connection to the profession or field under scrutiny" (Wilson, p. 156). Warnock was in this way, appointed to lead an inquiry into science and medicine, "where doctors and scientists had long been recognised 'as key holders of expertise'" (Warnock, cited in Wilson, p. 156). This "subtle but important change in British politics" (ibid), however successful it was as a formula for retaining a role for Establishment figures in providing regulatory advice to government, in effect did not represent all 'stakeholders' equally, but skewed the balance of power towards a select few experts.

Penelope Leach, as we have seen, in this way commented on what she referred to as the "strange constitution of the VLA", where members were selected based on their relative ignorance of the embryological facts so that that the group would reflect the attitudes of a pluralistic public, actually gave an eminent scientist like McLaren a lot of influence as one of the few in possession of any scientific information, and with a lot of authority and experience to make this perspective heard (Leach, interview with author, 1 October 2018). Others, like Virginia Bolton, also commented on the great authority that McLaren had, given her position as an eminent scientist, but also as a result of her consistent involvement on the various committees that had comprised the discussions.

> Well I see her role as being the rationale, grand adviser, probably the most knowledgeable – the person everyone would refer to as being the senior person who knew the most. So whenever she spoke everyone would listen because she was the authority … largely because of her intimate involvement with all the discussions

> that had taken place prior to publication of the Warnock Report. So we felt that she had insider knowledge. Because there were dissenting voices in Warnock and the decision about research, so she knew the kinds of arguments that she'd had to confront while that was going on. So she could then alert us to the sorts of things that people feared.
>
> Bolton, interview with author, 16 October 2018

Bolton's quote shows how McLaren, who was granted access to meetings and committees as the token, eminent scientist, was increasingly regarded as an expert not only as a result of her scientific expertise, but as someone who was able to relay and represent the voices of dissenters, of a wider constituency. McLaren then, functioned as a covert moral-expert, based on her experiential knowledge of the voices and issues that needed to be addressed. The above shows how McLaren was able to use her position as a scientist to orchestrate the debates, moving between various positions in order to align them – she used her expertise to build consensus. In contrast to Wilson's (2014) interpretation, McLaren was granted considerable influence as a result of her scientific expertise and is seen to appeal repeatedly to her role as a scientist in order to influence the debates. This was not a case of eschewing claims to expertise, but a process of humanely, compassionately, arriving at a position in which a public desire for healthy babies was reflected in the scientific research agenda, in a language that they could understand. It was an expertise of knowing how, under the remit of the authoritative scientist, to integrate various perspectives to find a consensus position: using science to do policy.

There are two faces to this coin. On the one hand, McLaren was able to draw on her practical and experiential knowledge of the views that needed to be addressed, unrestricted by the epistemological restraints of a particular discipline, precisely because of the space opened up by the absence of any official moral experts. This allowed her and others to shape the discussions in an unprecedented way, resulting in an Act that has been resilient and successful. Nonetheless, as Leach's comment suggests, McLaren was able to occupy this space more easily, leveraging her scientific authority to represent a constituency on issues that extended beyond her field. Others, who did not benefit from her professional eminence or experience, were not equally represented, despite the 'pluralistic' constitution of the committees.

This analysis, then, adds an important precondition to the efficacy of the 'pluralistic' committee. If the constitution of these committees so central to British policy-making is supposed to be pluralistic, this analysis begs a reassessment of what that means. This

observation falls in line with much of the critique made in the literature on the management of universities in the social sciences that critique the representation of 'society' on their committees. Committee structures that are posed to represent the undefined category of 'society' easily result in superficial speculation on this society's needs, or, at worst, the appropriation of their voices (Levin and Greenwood, 2016; Wright and Greenwood, 2017). Instead, these scholars claim, engagement with society gains meaning when the public is involved in shaping the conversation and the issues addressed by governing bodies from early on – in other words, they must be involved in constituting a language that adequately represents the issues at stake to the public the committee purports to represent (ibid).

Lay-representation serves no purpose unless all members are in possession of a common language. This is a language that McLaren tried to cultivate, trying to make her biological model speak to public sentiment, as well as her own sense of what was right. McLaren and her use of the biological account in negotiating these concerns provides a model, which suggests that a more explicit use of the biological facts in response to and guided by public concerns from the outset fulfils more fully the imperative for inclusivity. The model of superimposing domains relies crucially on a shared language, and that language, I suggest, should extend to an understanding of the role that science plays in arriving at social decisions if it is to be mobilised in aid of more broadly representative decision-making in the committee context, and beyond. This account of the various concerns, epistemologies and voices that McLaren incorporated into her case, should serve as a prompt to think through how these might be more adequately and formally represented in these ongoing deliberations, a point to which I return in Chapter 4.

A second important critique of Wilson's emphasis on neoliberal expertise as a common denominator between Warnock and Thatcher is that Warnock's project, as exemplified in the way that McLaren carried it forward, was a deeply social project. This case increasingly came to represent a common ground for a shared moral feeling, a translation of the concerns at the heart of public and scientific attitudes to research. This diverges considerably from the neoliberal position which posits that there is no such thing as society, only individual consumers or, in Thatcher's case, "individual men and women and their families" (Thatcher, 1987, interview with *Women's Own*, Thatcher Foundation Archive THCR5/2/262).

Both McLaren and Warnock, and the pro-research lobby that surrounded them, in contrast, were very much oriented towards what Warnock herself saw as "a steady and

general point of view" that would reflect a social idea of good, "the minimum requirements for a tolerable society" (1985, p. 1). The 14-day limit that they recommended in this way stood for the very idea of social morality itself. Considering how different these social visions are, we need to look further for the resonances between Thatcher, McLaren and Warnock, a question to which I return in Chapter 3, after a more thorough interpretation of McLaren's social vision for science.

The value of clinical translation: Consensus, pragmatism, moral good and social benefit

We have seen in this chapter how McLaren used her role as a scientist to advocate a specific research agenda and also how this consensus was cultivated increasingly by advocating and describing, scientifically, a set of clinical goals. This was not about persuading 'fundamentalists', or even convincing people to abandon a set of principles, but about showing them how their values, the values that, as Bolton's comments showed, scientists themselves held too, would be enacted by permitting the research needed to secure the potential benefits, efficacy and safety of IVF and PGD for the benefit of society at large.

Again, as we have seen, what I call Warnock's 'sociological pragmatism', the principle that the Committee's recommendations would never satisfy everyone, that legislation would merely have to be acceptable to most people so that the law would reflect the "minimum requirement for a tolerable society" (*A Question of Life*, 1985, p. 3), was implemented by McLaren in a model that was geared towards showing people who were in principle amenable to research , provided that the ends justified the means, the potential benefits of clinical medicine, also explaining to them the basic research that would be required to support such a programme. This was a pragmatic position in several senses: firstly, in the sense that there was no attempt to convince people who believed life started at conception, instead focusing on a strategy that was likely to lead to a workable outcome, but also because practical, clinical applications were posited as tangible goals over which a broad public could unite, a consensus that would only be consolidated by the 'proof' provided by the actual implementation of a clinical service. The scientific language of clinical applications, in this way, provided a pragmatic alternative to moral debate, redirecting discussions to what could be done for the good of society. We might return here briefly to what Warnock has written about her pragmatic logic, in order to understand how this relates to the arguments McLaren went on to make for clinical translation. Warnock

refers to the way in which the pragmatic need for legislation was complicated, and had to consider the moral sentiments of a broader public:

> And so, all the deliberations of the Committee were restricted, though not always explicitly, by a kind of pragmatic framework [to provide workable legislation]. At no time could we allow ourselves to indulge in idealism (and this was something that some members of the Committee found it quite hard to accept). Yet, on the other hand, our recommendations could not be too overtly practical or pragmatic. They had also, as far as possible, to be acceptable to society as a whole, and a society increasingly conscious of the moral dilemmas involved. It was for this reason that we called for, and paid serious attention to, evidence from a number of different organisations and individuals, and this evidence came in in great quantity.
>
> Warnock, 1985, pp. 505

A pragmatic approach provided a way for Warnock to avoid idealism but had to be augmented with a certain sensitivity to the morals of society at large through the collection of so-called 'evidence', or statements by special interest groups. The 14-day limit, in this articulation, came to express the limits to this pragmatism in the form a social contract, a judgement that was not pragmatic or utilitarian "but intuitive and, perhaps, sentimental" (Warnock, 1985, p. 518). Yet as McLaren carried the case forward, this social awareness came to be placed not in opposition to the pragmatic project, but as part and parcel with its implementation. This amalgamation is expressed in the idea of clinical translation, itself a practical aim, which thereby encompassed both the need for social consensus, for a shared idea of moral society, as well the legal necessity for clear limits to practice.

For McLaren, social values were integrated with pragmatic research goals and limits, again highlighting the unusual way in which she brought her science rationale and social values together. Clinical translation, in this way, came to represent a sociological pragmatism, in the form of a shared research agenda among scientists, which in turn reflected a general consensus on a moral position based on the idea of social benefit, all of which came to be expressed in the increasingly real clinical application of PGD. In terms of the fact/value debate in the STS literature described, clinical translation here embodies a plurality of values: the values of a moral code based on social benefit that reflects the democratic idea of consensus, but also a pragmatism that, rather than fighting abstracted political or idealistic feuds, is geared towards finding workable common ground. As we will now see, McLaren drew on these values as she made her scientific case on the road to PGD.

A point here about terminology: I call the approach carried forward by McLaren,

'sociological' as opposed to, for example 'democratic', which has often been the context stressed in analysis of the 1980s debates around biotechnology (e.g. Jasanoff, 2005). While I am aware of the deep history of philosophical pragmatism in the evolution of sociological methodology by the hands of figures such as by George Herbert Mead (1964) and its influence on the Chicago School and symbolic interactionism (Joas, 1993), I use the term 'sociological' initially in the broadest sense, to connote the systemic framing of the ethical problem at the heart of the HF&E debates by Warnock and then by McLaren. In contrast to the arguments of the anti-research lobby, I have shown how the pro-lobby case was framed as a question of social good based on the idea of consensus and brought about through an argument that relied on overlapping knowledge domains. This framing, then, made the ethical question a social problem of the development, structure and functioning of society and was pragmatic in that it looked for the best way forward given this fractured underpinning.

Moreover, McLaren's model is based on an epistemology that resonates with that of the interpretive school outlined in the introduction, in that meaning is constituted in relation to and through the self-conscious interpretation of a specific domain as an analogue for the question at hand – it is '*meaning for*'. In foregrounding the interpretive filters through which she arrives at her biological model, McLaren nonetheless makes a case in reference to a particular clinical application. The relevance of a translation in this method is determined by the "experiential or practical consequences of its application", very much within the pragmatist's paradigm (Haack, 1996, p. 643), and also within the paradigm of interpretive social science in which "understanding any action is analogous to textual interpretation" (Ricoeur, cited in Rabinow and Sullivan, 1979, p. 100), in which the intelligibility of any action requires reference to its larger context, a cultural world. It is thus primarily the social framing of the question at the heart of the debates, which evolved through the repeated asking of this question that sociological pragmatism here describes: a process of iterative engagement with domains in order to build a social case.

This point about definitions also serves to highlight the extent to which the pro-research use of the term 'pre-embryo' needs to be understood as more than a strategic response to the anti-research embryo in need of protection, as has been argued by Michael Mulkay (1997). While it is clear that a different ontological distinction was made, this distinction was premised on a social argument that shifted the locus of concerns from the status of the embryo to the question of what science could do towards the future betterment

of human health. This was, then, a pragmatic renegotiation, in the sociological sense, of an entity in relation to a new set of domains that presented an ethical case as a social question.

It is in this sense that my analysis in this chapter and the next, provides an important addition to previous analyses that skirt over the precise negotiation of the pro-lobby's methodological model, as I show how a change in the function of biological facts in the argument reflects an underlying change in the way in which biological knowledge was used and understood, which in turn, has implications for the ways in which ethical decisions were made over the course of the debates on human embryo research.

Conclusion

In this chapter, I have shown that McLaren became increasingly consistent in her use of the term 'pre-embryo' as it became part of a tight pro-research lobby case consolidated over time. This case came to focus on the ways in which the 'pre-embryo' could be used towards clinical applications. This meant that, as the main scientific authority in the public and policy discussions on human embryo research, McLaren's remit as a scientist changed from one of commenting on a narrow set of biological facts that could be used to substantiate a legal distinction to, much more broadly, framing the biological facts in an account that incorporated the values of scientists and a broader constituency of bettering human health, by positing specific applications along with a specific model relationship between basic research and clinical translation. In the next chapter, I will detail what this meant for McLaren in terms of the work she had to do as a scientist by describing her role in the development of PGD. The case of pre-implantation genetic diagnosis made the argument for the 'pre-embryo' as a separate and researchable entity even stronger, substantiating the biological definition by giving a concrete example of how the entity could be used to improve the lives of real human children. This clinical application therefore became central to the work McLaren was doing in layering clinical translation onto regulatory provision, social views, the feelings of scientists and non-scientists, and a specific view of development – in sum, building the biological picture in order to find common ground. This picture adds to previous accounts of the role of scientific authority in the debates (Crowe, 1990; Jasanoff, 2005; Lee & Morgan, 2001; Mulkay, 1997; Spallone 1987), showing more precisely how scientific expertise functioned. This analysis diverges from these accounts in that the 'pre-embryo' here represents not a distinguishable set of scientifically authoritative facts that were part of the process of "imposing on the debate

the cultural authority of science" (Mulkay, 1997, p. 115), but instead part of a deliberative, iterative model that included, from the onset, broader societal concerns that relied on a form of scientific expertise that incorporated these into their rationale.

Chapter 2

Pre-implantation genetic diagnosis: Getting behind the working model

We saw in the previous chapter how the prospect of clinical translation featured increasingly strongly in the pro-research case, consolidating an embryological account of development by showing how research on these early stages would translate into real applications for the benefit of human health. Clinical translation became part of a shared scientific agenda, but also represented a shared moral compass between scientists and potential patients, representing the positive improvements human research could deliver at the cost of working on embryos. Clinical translation was thereby used to consolidate the idea of the 'pre-embryo' as a distinguishable, researchable entity by providing examples of how this discrete stage in development could be positively exploited. The use of clinical translation as an argument for research also prescribed a new role for a basic scientist like McLaren, as one of illuminating the relationship between basic research and clinical applications in order to relate to her target constituency over a shared feeling of what was right. The story of PGD shows how McLaren was aware from early on not only of the technique's potential to demonstrate the benefits of research on *in vitro* human embryos but also of the particular resonance this would have with a broader constituency in principle open to the use of human embryos towards a desirable end, but in need of tangible outcomes on which to hedge their bets. PGD increasingly became part of a working model of science and policy that was being consolidated through tangible techniques, and provided a clear example – in answer to the Warnock question – of how the human embryo could be used.

PGD quickly became a technical possibility over the course of the debates (Franklin and Roberts, 2006). The technical hurdles preventing its feasibility were suddenly resolved: first the amplification of DNA by Karry Mullis working for the Cetus Corporation solved the problem of using material from a single cell, then the possibility of applying single-cell molecular analysis to the development of PGD by Marylin Monk at McLaren's lab who

then collaborated with Alan Handyside and Robert Winston at Hammersmith Hospital on the biopsy of single cells from embryos to effectively diagnose defective embryos in a mouse model for Lesch-Nychan disease (Monk et al., 1987, in Franklin and Roberts 2006, p. 51), then the application of Mullis' method of PCR to PGD by Monk and her colleague Cathy Holding to diagnose beta-thalassemia in a single cell from eight-cell embryos from a mutant mouse lacking this gene (Holding and Monk 1989, ibid.), and finally, with Peter Braude in Cambridge, showed it was possible to diagnose sickle-cell anaemia in a single polar body of the human egg (Franklin, 2006, p. 52). In light of these developments, Franklin writes that McLaren quickly shifted from expressing reservations about the technical hurdles to achieving PGD to claiming the technique was imminent, and the rest of the scientific community followed over the rest of the debates (Franklin and Roberts, 2006, p. 47). McLaren was now also quick to advocate PGD's therapeutic use in the context of the debates. As we shall see, however, this advocacy did not consist merely of conveying the technical possibility, but also of framing the technique to make it appear both desirable and ethical from early on.

McLaren's role in the PGD story brings to light an important aspect of the discussions on biotechnology, to which Franklin also refers in her account of PGD vis-à-vis the work of Professor of Science and Technology Studies Sheila Jasanoff, namely how public discussions on biotechnology are always 'framed' to centralize certain questions and agendas. By showing how the question itself and the concepts drawn upon in these deliberations emerge from a background of "partiality' and "plurality" (Jasanoff in Franklin, 2006, p. xviii), Jasanoff's concept of framing shows how these perspectives may "partially overlap or diverge, they may stay the same or change, and they may be more or less recognisable or comprehensible to other social actors" (ibid.).

In contrast to political agenda setting, then, this view begs a more detailed description of the concerns brought into and prioritised in policy discussions. McLaren's effort to carry forward the Warnock model, and how she built on this to incorporate new concerns and emphasise a specific question, can contribute to the important process of showing "the diversity of material with which they [frames] are constructed, how they achieve their taken-for-granted status, and what happens to make frames change" (Jasanoff cited in Franklin, 2006, p. xix), in order to evaluate how and why this model may or may not serve its purpose. The account of McLaren's role in developing the case for PGD also shows how McLaren as an individual framed the discussions, bringing into purview a

specific question and set of relationships. Indeed, McLaren used her 'unusual' position from early on – on Warnock and even before – in order to frame the ways in which the case for PGD would be made, feeding into Sarah Franklin's insight that the prospect of genetic diagnosis was always already there as IVF was being developed.

McLaren was aware of the role that the technology could play in persuading potential patients that human embryo research was necessary and framed the technique in order to address public concerns with regard to genetic diagnosis. This common ground came to be reflected and consolidated through a shared biological model of development that was assisted, while also making a clear delineation between 'pre-embryo' and embryo-child. This account differs slightly from that offered in other analyses of the debates that separate the role of the 'pre-embryo' from the arguments for PGD in the pro-research case (Gunning and English, 1993; Jasanoff, 2005; Mulkay, 1997). As we shall see, I add to this analysis of the factors that contributed to the victory of the pro-lobby, showing how PGD and the arguments for the 'pre-embryo' related. The story of McLaren's role in framing PGD for the pro-lobby case also helps describe her contributions as a scientist specifically, addressing the question of what biologists can offer to public debate, and also, importantly, what is at stake when we implicitly entrust this to authoritative individuals.

Framing PGD in-the-making

Theodosiou and Johnson (2012) and Franklin (2006) have both emphasised how the development of PGD mapped exactly onto the period of the debates on human fertilisation at embryology and suggest that the development of the technique was to some extent spurred along by the urgency of developing a convincing pro-research case. Franklin (2006) in this way points to the "role of PGD in focusing and clarifying public attitudes towards reproductive biomedicine" as it was being developed over the course of the HF&E debates (p. 39). Part of this significance, she goes on, derives from "its transformation from being a scientific possibility into a clinical reality – or 'birth' – during exactly the time period of legislative 'gestation' of the Human Fertilisation and Embryology Act. Namely 1984–1991" (ibid). Theodosiou and Johnson (2012) additionally suggest the strategic significance of focusing on genetic screening in the pro-lobby case, and they coin the phrase 'genetic embryo' to "encapsulate a variety of ways in which the genetic make-up of the embryo was a subject of debate and influenced the approach taken to the permissibility of scientific techniques" in Britain, helping them to explain why the pro-research lobby

came to focus their case strategically on genetic disease in the embryo research debate, building on the resonance from prior usage in the abortion debate in Britain (pp. 40–1).

McLaren was a prominent figure in conceptualising this 'genetic embryo', specifically in relation to PGD as it began to emerge on the clinical horizon in the lead up to the HF&E debates, and then in explaining, advocating and facilitating the development of this technique over the course of the debates. In doing so, McLaren was part of the process of 'focusing' and 'clarifying' that Franklin describes.

I will start by referring to an early statement by McLaren on the prospect of genetic diagnosis, and how this reflects her own ethical position on human embryo research. I then go on to show how she builds on this ethical position in the context of the Warnock Inquiry and the ensuing debates, by incorporating an understanding of social attitudes into a model relationship between basic and applied research. As in the previous chapter, these relationships and perspectives shape the embryological account given, leading to an altered developmental narrative that comes to represent a common moral ground over which a pro-research lobby could unite.

PGD pre-Warnock

McLaren was one of the early proponents of PGD. Franklin and Roberts (2006) write that McLaren had been influenced early on by the work of "two of her close colleagues at the University College Hospital (UCH) to view pre-implantation diagnosis as an important research priority" (p. 44). One was Bernadette Modell, a geneticist working closely with the Greek-Cypriot community trying to reduce the incidence of thalassemia, and the second was Marylin Monk, a molecular biologist in McLaren's lab who had already begun to develop the sensitive single-cell molecular analysis that later provided the basis for the first successful PGD. These scientists, Franklin claims, shared an understanding that patients should be helped in their wish for "an earlier means of diagnosis that would assure them of being able to begin a pregnancy free of … debilitating disease" (ibid.). Yet, even before PGD was becoming a clinical reality, McLaren was already practiced at framing the possibility of PGD. Theodosiou and Johnson (2012), in their account of the role of PGD in the pro-research lobby case during the HF&E debates, have shown how McLaren pushed for a shift in the policy of the Medical Research Council (MRC) towards research into genetic screening techniques for abnormalities in the late 1970s and early 1980s (p. 60). The MRC, initially cautious of research on, or transfer of, IVF embryos viewed these

techniques as purely experimental and dangerous. Indeed, McLaren herself even cautioned in 1971 that "a malformed baby" might be born if transfers were "carried out prematurely, through the desire … to be first in the field" (McLaren letter to the MRC, McLaren papers, British Library).

The MRC, however, was forced to change its policy following the birth of the first IVF baby in 1978, which demonstrated the clinical success of IVF (Johnson and Theodosiou, 2012, p. 49). McLaren's previous caution towards the techniques of IVF still remained, but was framed differently. In a letter to Dr Barbara Rashbass in the same year (1978), expressing her view on *in vitro* fertilisation and embryo transfer to the Council, McLaren writes that she "thinks the use of *in vitro* fertilisation for research purposes should be encouraged" and that she "would like to see this recognised by the Council", suggesting that "perhaps a review of policy in this field would be no bad thing" (July, 1978, FD 13/242, The National Archives, Kew). In making the case for such research she outlines several important lines of inquiry in the field, including testing sperm for its penetrating power and developing understanding of the pre-implantation period in the human embryo, such as the production of human chorionic gonadotropin (HCG), questions which would have implications for early pregnancy diagnosis, oocyte freezing, and testing for abnormalities in the embryo prior to transfer. Regarding the last point, she writes,

> If embryo transfer is ever to be used clinically, we need to be certain that embryos derived by *in vitro* fertilisation are not abnormal. The only ethical approach to this question is to carry out chromosomal and biochemical studies on a series of such embryos during the pre-implantation period. Such studies might well also throw light on the etiology of spontaneous abnormalities of implantation and early development.
>
> McLaren to Rashbass, 3 July 1978, The National Archives, Kew, FD 13/242

As Theodosiou and Johnson (2012) point out in this letter, "the fear that IVF might cause abnormalities … was suddenly used as a justification to encourage, rather than withhold, support for research on human embryos" (p. 50). Theodosiou and Johnson go on to show how the MRC came to gradually adopt this approach so that, by 1982, its advisory group supported the use of IVF techniques in the "fields of embryology and inherited disease" (p.51), encouraging the pursuit of a screening device to determine the chromosome constitution of an embryo, and adding the genetic expertise of several scientists, including McLaren, to the Group, marking a shift towards the use of genetic arguments to justify research on the human embryo (p. 51). This leads them to show how a new position was

taken up by the MRC: while at first the fears of abnormalities that may be caused by IVF inhibited the development of the concept of the genetic embryo, eventually, "the fear of elusive IVF abnormalities was transformed into a desire to pursue, rather than impede, research on genetic abnormalities in IVF embryos" reflected in the "medico-scientific rhetoric from 1978 onwards, and was used by the MRC to justify a radical policy change on human embryo research" (Theodosiou and Johnson, 2012, p. 51).

McLaren's reconceptualization of the embryo in the 1978 letter therefore indexes a crucial shift in attitude in the scientific community towards human embryo experimentation that can be seen as a predecessor to those used by the Warnock Committee in favour of research. Genetic abnormality was increasingly cast as a characteristic of natural biology, justifying a greater degree of intervention in the reproductive cycle, as well as the research programme to support it. McLaren here already justifies research by referring to the prospect of genetic diagnosis – basic research is presented as first and foremost an inextricable component of clinical translation while any basic scientific information this research might yield on the 'etiology of spontaneous abnormalities of implantation and early development' is only secondary. Basic research is framed as part of a clinical project, meaning that any description of development is always given in reference to a specific application. This changes the status of the biological facts from objective or definitive, to relative and pragmatic, as Franklin argues in her *Biological Relatives* (2013a), an argument also made by Strathern (1992b) and Thompson (2013) in the context of the public debates on reproductive technologies and biomedicine.

The description of McLaren's role that I have given shows that this reframing of the biological facts of development in a clinical context also had implications for the way that development was communicated to and interpreted by non-scientists when the account was taken forward and developed over the course of the Warnock debates. As we shall see, it laid the rudiments for the arguments for human embryo research based on a clinical need for genetic diagnosis that is reflected in a biological model that is not natural but assisted.

The clinical framing of the need for research used by McLaren in 1978 also indicates how McLaren would come to position herself in the debates. McLaren makes a judgement on the permissibility of research that she feels comfortable making in the context of the MRC discussions with other scientists, but which she is later hesitant to make in the context of the Warnock Inquiry. In *Where to Draw the Line* she expressly refuses to comment on the purposes to which human embryo should be put, offering only a scientific

account punctuated by landmarks to inform a broader discussion. The letter to Rashbass, however, shows how McLaren here feels able to comment on ethics within her remit as a scientist. She does so by implication, by pointing to the desirability of a clinical application, thereby justifying an accompanying research agenda.

The inextricable relationship between basic research and clinical application as McLaren describes it in reference to genetic diagnosis not only facilitates her making ethical claims as a scientist, but also helps to explain her strong moral conviction that embryo research is justified. In an interview conducted for this study with Lord Robert Winston, who was another active member of the pro-research lobby, especially in Parliament, and who ran the unit at Hammersmith that saw the important first successful use of PGD to diagnose mice for Lesch-Nychan disease in 1987, he conveyed the emotional component of McLaren's view on human embryo research:

> You know what? 'Uncharacteristic' is interesting, yes. I think in some respects, yes. I mean she could see that by the nature of how biologists worked on the whole field, there's a big difference between the implanted embryo and the pre-implantation embryo, and that's what she was really trying to identify. The pre-implantation embryo has no real confirmed trajectory, and she saw that very clearly. And interestingly, other biologists didn't see that in the same way. I mean even though we were using, for example, the contraceptive coil … but then of course some of those were probably after implantation, and we were using some forms of oral contraceptive that were probably destroying embryos as well. But I think Anne felt, really felt quite aggrieved that they were treated in the same way – I think there was an emotional element in her thinking about it.[4]
>
> Winston, interview with author, 26 September 2018

Winston here describes what he calls an 'uncharacteristic' use of a divisive term, the 'pre-embryo', by the usually measured McLaren. He explains this by referring to McLaren's deeply emotional conviction that the early embryo and late embryo are different and shows how this emerged from a particular view of development, but also a broader perspective that saw the status of the embryo in relation to the clinical applications towards

4 It has to be noted that Winston's characterisation of McLaren as somehow emotional, as opposed to the rational Powell, is read with some unease. There is a history of women being cast as sentimental in a male-dominated science in order to delegitimise their perspectives as 'unscientific', thereby perpetuating a long-standing hierarchy of male, rational, and objective over female, sentimental, and subjective. It is thus with some trepidation that I approach Winston's comments, but I hope that they serve to add to the characterisation of McLaren's unique approach that I have offered thus far.

which it was being used across the field of reproductive biology. As in 1978, McLaren seems to arrive at a moral conviction that is strongly felt, as a result of a particular envisioned relationship between research and application. In an interview conducted with her in 2007 by Johnson and Franklin (interview Anne McLaren, British Library Mammalian Developmental Biology, 2007), McLaren herself describes the sense in which her vision of clinical application as part of a research project led her to a particular moral stance:

> And I remember being, it hadn't occurred to me that there would be any ethical difference felt between embryos that were made for reproductive purposes and then not needed for the original purpose and embryos that were made directly for research. It just hadn't entered my mind that there might be some ethical difference. And certain people on the committee … came out with this, it was almost a slogan cause they said it over and over again, you would just be "creating in order to destroy". And I felt that one wasn't actually creating, what one was doing was inseminating a donated egg. And one wasn't just destroying, one was doing research which would lead to benefits.
>
> McLaren, interview with Franklin and Johnson, British Library, 2007

McLaren's moral position again derives from her broad vision that incorporates clinical and basic science under the same goal: "doing research that would lead to benefits." Her moral stance and conviction become especially clear in the context of discussing clinical applications. In her ensuing contributions to the more public debate on human embryo research, McLaren finds ways in which to impart her 'emotional' sense that human embryos should be used towards clinical goals, by framing her descriptions of development increasingly in the context of the clinical prospect of genetic diagnosis. This, then, represents a kind of moral pragmatism, much in line with the approach taken by Warnock, that was geared towards concrete solutions and applications at the same time that she kept in purview broadly held social attitudes, which remained intuitively attuned to a shared sense of where the limits should lie. For McLaren, making the case for PGD similarly continues to be both a rational case for a specific application, but also an iterative process of incorporating the sentiments of a wider constituency, as well as her own, into her biological model.

Framing PGD for a wider constituency

On the Warnock Committee, McLaren was faced with a very different task to the one she had on the MRC. On the MRC, the response to genetic screening had to be made to

determine a research agenda and policy within a professional community of scientists. On the Warnock Committee, the question of genetic screening had to be addressed in the context of advising the Government on how to treat the human embryo and would thus have to reflect the views not just of embryologists, geneticists and reproductive biologists, but of society as a whole. Nonetheless, as in 1978, we see here how McLaren takes on a more definitive moral stance in the discussions of a specific clinical application, providing her with a scientific issue through which to assert a moral imperative. It is also by tracing McLaren's arguments in favour of PGD, that her ethical model comes to light.

In an interview conducted in 2018 for this study, Warnock recalls that McLaren was indispensable to the inquiry on the issue of genetic engineering. Indeed, besides the introduction of a very particular scientific account, the issue that Warnock highlights, to which McLaren became instrumental, was the question of future applications of human embryo research. Warnock explains how McLaren, "really beyond her call of duty, was extremely helpful with the whole business of drafting and circulating potential drafts for discussion" (Warnock, interview with author, 21 August 2018). She explains how McLaren helped her clear what she describes as a 'fog' in her thinking about the issue:

> And I used to make a point to see her between meetings, so we could go in for drafting exercises. … Anne was amazingly helpful with helping to clear the fog. Clarity was her great gift really. And I remember, we'd been set up to look at infertility. But it become quite clear that we had to, or ought to really, incorporate a lot of stuff that really had nothing to do with infertility at all but that was to do with genetic engineering and looking to the future in that way. And I just couldn't see how to fit into this format of the report, because it seemed to be quite outside of our terms of reference.
>
> Warnock, interview with author, 21 August 2018

It was thus on the issue of future applications of embryo research for purposes other than infertility treatment that Warnock lacked the clarity she now seemed to have gained on infertility-related recommendations. The issue of genetic engineering had emerged from the so-called 'evidence' submitted to the Committee, which, as Warnock explains in her interview with me, was "of course not factual evidence but the evidence of … what people thought" (Warnock, 21 August 2018), which had been collected over the two years of the investigation from various interest groups. In these various responses to research, the issue of genetic engineering had proven to be a bigger concern than anticipated. The question of research, Warnock further recalls, had been pushed to the end of the Committee's deliberations, and so there was suddenly time-pressure on the process of incorporating

these unanticipated matters into the Report. McLaren, ever forward-looking and attuned to the potential developments in her field, as her former scientific colleagues all emphasised, was well-suited to the task. Warnock goes on to explain the nature of the problem as she saw it at the time:

> I think it became clear that if you … I'm trying to think of the order in which this occurred … that if you had an embryo, or several embryos in the laboratory alive, you could, at the very least select which embryos you were going to choose to implant, but there was also the possibility of actually intervening in the development of the embryo.
>
> Warnock, interview with author, 21 August 2018

The solution they found, Warnock explains, was to "find a way of devoting one chapter to the future, that I hadn't seen how we could incorporate but that actually flowed quite easily in the end – to ways of possibly intervening to change the embryos", that represented "future possibilities that really weren't anything to do with infertility but would be the next set of advances that would happen" (Warnock, interview with author, 21 August 2018). As the above quotation shows, the chapter came to centre around a distinction between techniques that depended on genetic manipulation, such as 'cloning' or the use of animal-human hybrids with trans-species fertilisation, and the therapeutic benefits of selection by assessing, for example, whether sperm is healthy in the hamster-fertilisation test, and selecting healthy embryos based on genetic analysis. Therapeutic benefits are repeatedly aligned with selection in the chapter, while genetic manipulation falls outside of this remit and is precluded by the Warnock recommendations.

In the case of the prevention of genetic disease, the chapter again states that "public anxiety about these techniques centres, not so much on their possible therapeutic use, but on the idea of the deliberate creation of human beings with specific characteristics" (Warnock, 1985, p. 75), and the Committee goes on to advise that research in this area be regulated by the proposed authority and that guidance be reviewed from time to time to accommodate new advances in addition to reiterating that the recommendations in the Report generally preclude advances this this area. The chapter projects an image of the future of embryo research in which genetic diagnosis is geared towards the goal of producing healthy offspring by means of selection, a goal that is identified as one that the public can get behind, while the line is drawn at genetic manipulation, which is identified as the real source of public concern around genetic intervention.

Warnock says that she was impressed by how McLaren was able to contribute a

way of thinking, as opposed to scientific expertise alone, to the discussion:

> But what impressed me so much was that this was not a technical, scientific problem; this was a sort of stylistic, sort of structural problem, to do with the shape of the report as a whole, which wasn't her specialty, but which she was extremely good at seeing the point of and discussing. And I valued her help in that respect just as much as I valued her scientific input.
>
> Warnock, interview with author, 21 August 2018

This structuring and framing demanded not only that McLaren introduced relevant scientific information, but that she was able to distil the crux of the social concerns into a scientific account, once again showing how her discussion of science is shaped by social values. Yet, even further than this, McLaren is able to deduce these concerns and values, through her perception of feelings and to express in scientific terms her understanding of people's hopes. This is a process of persuasion based not on coercion, but on an understanding of the human values and how they are expressed. McLaren and Warnock worked together, drawing on various logics to inform the public by combining philosophical expertise, emotional appeal, and scientific reasoning. As in the case of the 'pre-embryo', they are superimposing by translating the sentiments expressed in the public evidence into regulatory lines imposed on scientific practice and future research directions.

Almost all of McLaren's former colleagues whom I interviewed for this dissertation, both from her scientific and policy circles, have emphasised her clarity of mind as a key attribute. Warnock's description of her contributions to developing a response to genetic engineering begins to show what this 'clarity' allowed her to see. The assistance McLaren was able to provide in addressing the question of genetic engineering seemed to rely on an understanding of the source of public anxiety which was then used to structure the scientific information in such a way as to address these concerns. The process then, required both empathy as well as the ability to develop a rationale that would come to reflect this. Winston refers to this relationship between affect and rational thinking in McLaren's approach:

> I don't think if you'd said to him [Enoch Powell], "what are the four ethical principles" … Anne would have said immediately, "Well, you respect the autonomy of the individual; secondly you have this idea of trying to good; thirdly, not to do harm; and lastly to have a just solution to what you're doing". Powell couldn't have answered any of that stuff. For him it was a kind of gut feeling he had. And Anne ultimately, I think, was actually an academic, with an academic view, but with a wonderful humanity about her which was actually always there. And that for me was something I always remember, when she talked to my PhD students or

> whatever she was doing, you felt this was somebody who was feeling her way through things.
>
> Winston, interview with author, 26 September 2018

Winston, although perhaps lacking the clarity that Warnock has, attempts to convey the same sense in which McLaren's approach was so distinctive because of the way in which she brought scientific rationale and sentiment together. He contrasts McLaren's ethical approach to that of Enoch Powell, showing that, unlike Powell, she was able to justify her ethical position by referring to certain principles. Powell, on the other hand, merely had a 'gut feeling'. Yet Winston concedes in the same passage that McLaren's moral position was also emotionally driven. The real distinction Winston is trying to make, it seems, is that McLaren was able to explain her 'gut feeling' in her own professional language.

As she did in Warnock's example, McLaren performed this translation by drawing on scientific information and by making distinctions, structuring the information in such a way as to bring to light the ethical implications of the science in a particular context of use. Integral to this process was 'feeling her way through things'. Winston's passage also gestures to McLaren's dialogical methodology: that she spoke to people in order to develop her views. This, too, as we have seen in Warnock's account, allowed her to incorporate an intimate understanding of others' views into a shared moral position expressed in scientific terms. Once again, we see how McLaren's colleagues describe her as effective in the policy context precisely because she was able to bring the traditionally disparate domains of facts and values together; both are encompassed in her remit as a scientist in public debate.

Winston goes on to describe a particular instance where McLaren's scientific and moral clarity was important, as they collaborated on translating PGD into practice:

> Anne wasn't really interested in the parliamentary frippery – she was never of that mind, she was really interested intellectually in what was going on much more. But she was astonishingly clear about what should happen and what was happening. And I suppose right through my relationship with her, right up to the point when it became clear that we were going to get PGD off the bench and into the patient, there was then talk of course about how about modifying the human embryo. *And Anne of course said, that's not necessary, you don't need to modify it because pre-implantation diagnosis makes that an unnecessary thing, and she was very clear about that actually.*
>
> Winston, interview with author, 26 September 2018 [emphasis added]

In describing McLaren's response to the parliamentary debates and her interest in

them as ‘intellectual’, Winston points to the way in which she was able – in Parliament just as on the Warnock Committee – to integrate the views she encountered into an overarching framework expressed in a scientific account. This ‘intellectual’ perspective allowed her to see, when PGD was being debated, that it rendered genetic modification unnecessary, an argument that she knew, as we have seen from the Warnock anecdote, would alleviate anxiety amongst some of the public. Thus, it was an intellectual sense of clarity that allowed her to incorporate emotional concerns into a scientific world view. This affective translation, this integration of emotional feeling by showing its implications in scientific terms, encompasses both McLaren’s own feeling of what is right as well as sense of compassion, an understanding of the hopes and values underlying the sentiments expressed by others. This ability to integrate concerns in the form of distinctions and framings is reflected in McLaren’s treatment of PGD in the public debate and, I will now show, meant that the common moral ground she was constructing for a growing pro-research alliance also comes to be expressed in a new biological picture.

PGD and the public role of the scientist

As genetic disease became increasingly central to the parliamentary and public debates, it became urgent that scientists clarified the possibilities and limits to genetic intervention in the embryo (Mulkay, 1997). In a commentary piece published in Nature in 1985, McLaren, writing with H. John Evans (Director of the MRC Clinical and Population Cytogenetics Institute in Edinburgh at the time), attempts to do just this. The article is a reaction to the introduction of Enoch Powell’s Unborn Children (Protection) Bill into Parliament, a bill that many of the scientists involved in the pro-research lobby still refer to as an important galvanising incident, making clear the extent of the opposition to research that they would have to face. The central issue of debate had now clearly moved to human embryo research and this was being debated largely in terms of specific genetic therapies. The article is also representative of a shift in Nature’s policy noted by Melinda Baldwin (2015) in her history of the journal, in which she shows how it became increasingly a site for public debate in the late 1970s and 1980s. Authors would use an article to say their piece, from a scientific perspective, but clearly in order to elicit support for a political agenda. This political case, made in the capacity of a scientist in a public debate, then, is made through the language of clinical applications.

The article immediately frames the debate as one about intervention in the embryo

in order to alleviate genetic defects. The article opens, "Much to the dismay of most informed scientists, infertile couples and parents are at risk of producing children with severe inherited genetic defects" (McLaren and Evans, 1985, p. 127). It continues:

> Unfortunately, the bill will … prevent attempts to circumvent the production and transfer of severely genetically abnormal embryos, a possibility that is almost within the grasp of medical science, and which is ignored or rejected by proponents of the bill.
>
> McLaren and Evans, 1985, p. 127

McLaren and Evans are responding particularly to the arguments made by the French geneticist Professor Jerome Lejeune, who addressed the pro-life group with his views on curing inherited genetic disease. His proposed alternatives to genetic intervention, in themselves flawed according to McLaren and Evans, are also all concerned with therapy. It is here that McLaren's distinction between selection and manipulation of the genetics of the embryo is made again:

> Defining and understanding the nature of the defect can lead to rational attempts to alleviate the disease, by supplementation (à la Lejeune) or by circumvention ... these developments are somewhat removed from practicality and ... will be applicable only in a limited number of diseases. Moreover, *they are concerned with attempted amelioration of the disease and not with its prevention*. And they involve manipulation of the patient, not of the embryo.
>
> McLaren and Evans, 1985, p. 127 [emphasis added]

They go on to cite examples, such as the case of Duchenne muscular dystrophy, in which only a small proportion of the embryos will be affected, making detection of "which is normal and which is abnormal" a viable therapeutic route, and dispensing with the need for "genetic manipulation of the embryo" (p. 128). At this point, PGD was still only on the horizon (Franklin and Roberts, 2006), but the technique, the authors claim, lies "just around the corner, and would require further studies which would become outlawed under Mr Powell's bill" (ibid). McLaren's earlier position in which clinical applications frame the need for research is again evident, but importantly, we here see how PGD provides a powerful example of such a clinical application that addresses a moral imperative that is shared throughout society to secure healthy children. In this way, PGD came to represent certainty: first in the form of a shared moral position based on a clearly defined clinical goal, and then, as a real clinical possibility providing certainty to couples having children, which in turn also validated an increasingly positivistic and deterministic account of development that therefore also came to reflect and reinforce the certainty that PGD

provided.

Here we see how PGD again provides a context in which McLaren is able to confidently assert a scientific and moral case for research that now also incorporates the morals of a wider public. PGD, like the Warnock model, thus came to represent another amalgam – of regulative provision that responded to social attitudes, of McLaren's morals, of clinical science and basic research, all brought together through an intuitive, affective understanding of people's values, concerns and hopes. This history provides another example of the process of superimposition that I have described, showing how McLaren's justification for research to better understand genetic defects in 1978 set in motion a research project in which abnormal embryos are used to better understand development, which is then referred to in the debates on human embryology to make the case that research can produce normal development. The increased conviction with which McLaren takes this public stance as a scientist is also reflected in a consolidated biological account, to which I will now turn.

The biological picture

As the debates progressed, PGD went quickly from being a theoretical future possibility, to an imminent, and then actual clinical procedure. The earlier model that posited a research agenda around the 'pre-embryo' aimed towards a projected clinical horizon, was thereby transformed into and confirmed as a working biological model. Increasingly, PGD showed that the early embryo could be screened and transferred without affecting the healthy development of the later embryo, thus confirming the existence of a researchable and conceptually separable early embryo. By legitimising the 'pre-embryo' as a scientifically separable entity, the clinical procedure thus also dispensed with the need for the rationale provided in *Where to Draw the Line* in the sociological language of landmarks. The "proof", as James Gowans, secretary of the MRC at the time of the debates, put it, "was in the pudding" (Gowans, interview with author, 18 September 2018). The certainty that PGD purportedly offered couples was thus reflected in the increasingly widespread acceptance of a biological model that set up the early embryo as a research tool. The status of the human 'pre-embryo' was defined in relation to a specific application, which was now a clinical reality, giving substance and certainty to the pro-research lobby scientific answer to the Warnock question of how to govern the treatment of the human embryo.

At the same time, though, the need for PGD was explained by a biological picture

that was distinctly uncertain. Embryo selection and transfer was only necessary due to the inefficiency inherent to development – the natural fallibility in a still poorly understood contingent biological cycle. This generates a tension between two developmental views: one that emphasises the certainty that genetic screening can provide through an account that suggests that mastering of the embryo's genetics is sufficient for healthy development, and another that emphasises the contingency of development upon a suitable environment. The latter is both the necessary precondition to making this research acceptable, while it also undoes the logic on which the procedure's viability is predicated.

Nonetheless, these two embryos are able to coexist in the dual developmental narrative used by the pro-research lobby because the case was structured according to the same scientific-moral stance expressed by McLaren in 1978, namely that clinical application and basic research should be conceptualised as part of the same project. This model is thus able to incorporate two models of development expressing two very different agendas. The first, the genetic one, is geared towards the achievement of a healthy pregnancy by the means available. The second, frames a research agenda around a still poorly understood phase in development. The genetic trajectory reflecting clinical certainty is supported by a broader vision that points to the need for the study of development to achieve certainty in clinical application. The latter vision becomes less necessary as the genetic view is, to some extent, confirmed by the success of PGD. This certainty is reflected in the use of the 'pre-embryo' in pro-lobby case, as in the McLaren and Evans article, framed by the need for genetic diagnosis, and no longer in terms of progressive physiological distinction mapped onto individuality that we saw in the previous chapter.

At the same time, the success of PGD also changed the clinical view of development that emphasised certainty through genetic selection. By translating the biological, contingent trajectory into something that can be secured and optimised through scientific intervention, the natural inefficiency of the cycle is implicitly confirmed, and so the biological model of the scientists is also validated. Heightened intervention in order to secure so-called 'normal development' thus paradoxically reflects an underlying biological view that is contingent. In terms of McLaren's diagram in figure 1, the spiral is always implicated by the unidirectional appendage of embryo to child.

This points to an interesting outcome of an approach that built a case by incorporating public sentiment by introducing new biological distinctions, of merging perspectives: namely that it alters the view on both sides, from both perspectives. By taking

the public need for reproductive certainty seriously, a biological model was validated in the public eye that acknowledged contingency. Similarly, by drawing the line at selection in response to public attitudes, a biological picture was instantiated in which artificial intervention secured a paradoxically more 'normal' developmental trajectory, a process Franklin (2014) has described as 'analogic return', drawing on Strathern, whereby analogies 'travel back' to remake both their object and its epistemology, changing even the context of the concept itself. The conceptual disruption of selection to the popular view of deterministic development is in this way arguably much greater with selection than it would have been if a case had been made for editing the genome.

Rather than an imposition on 'natural' biology, scientific intervention becomes part and parcel with the natural cycle – biology becomes assisted. By incorporating a popular view into the scientific account, the public, over time, is rendered more amenable to the biologist's vision; by taking seriously public concerns and providing a shared language in which to express them, the scientist's case becomes much more convincing at the same time that there emerges a common ground. In this sense, the 'pre-embryo' became part of not only of a shared 'approach', as in the quote by Virginia Bolton in the previous chapter, but also a shared, and altered way of seeing biology. The explanation of how PGD was used in the debates that I have given, thus also offers a new sense, or at least shows in more detail, how the 'pre-embryo' functioned in the pro-lobby case. While previous accounts of the debates have cited both the introduction of the term 'pre-embryo' and the promise of PGD as key factors to the pro-lobby victory (e.g. Gunning and English, 1993; Jasanoff, 2005; Mulkay, 1997) they have fallen short of showing fully how the two were mutually reinforcing: that is, how they were connected to form the pro-research argument. British sociologist Michael Mulkay (1997), for example, outlines several crucial factors to the pro-lobby success. The first, he says, was "the transformation of participants' understanding of the experimental subject of embryo research" (p. 132):

> The idea of the 'pre-embryo' helped to remove the moral barrier to the continuation of embryo research by convincing people and/or by helping them to express their conviction that this was not research involving real human beings, but experimental use of unformed biological material. … The great polemical advantage of the concept of the 'pre-embryo' for the pro-research lobby was that it placed the experimental subject of embryo research beyond the reach of its opponents' moral discourse.
>
> Mulkay, 1997, pp. 132–133

This interpretation is in line with other analyses of the debates. STS scholar Sheila Jasanoff (2005), in her analysis of the ways into the constitutional framings that shape policies for the life sciences, also refers to 'pre-embryo' as a "vital ontological distinction":

> By giving a name to the pre-14-day entity, research scientists and their political allies met and parried the nominalist tactics of the pro-life lobby. Thus, in a neat display of coproduction, the adoption of new rules for the ethical conduct of science resulted, at one and the same time, in the coming into being of a new natural entity.
>
> Jasanoff, 2005, p. 152

While Jasanoff acknowledges how the 'pre-embryo' distinction was negotiated in relation to a set of 'rules for ethical conduct', this remains an 'ontological', not an epistemological one, and she does not describe this process of co-production she evokes, which, as we have seen, occurred very much through the part that the 'pre-embryo' played in PGD.

Mulkay goes on to describe the possibility of PGD as a second, separate factor leading to the success of the pro-lobby:

> The second crucial element in the success of the pro-research campaign was its increasing emphasis on the possibility of controlling genetic disease without altering the genetic make-up of human individuals. The pro-research lobby convinced the majority of parliamentarians that many forms of genetic disorder could be more or less eradicated by means of genetic screening of IVF embryos. As a result, as the parliamentary debate entered its concluding stage, the defenceless unborn children of the early debates largely disappeared from view and were replaced by the miraculously healthy children to be produced in due course with the techniques forthcoming from embryo research. The replacement of the embryo by the pre-embryo made further research seem permissible. The acceptance of scientists' claims concerning the wide range of potential benefits to be obtained by means of genetic screening made the continuation of embryo research appear obligatory to the majority of parliamentarians.
>
> Mulkay, 1997, p. 133

We see that Mulkay posits PGD as a second, important factor that was distinct from the argument made for the 'pre-embryo' and that, in the debate, and served the distinct rhetorical function of making research 'obligatory' rather than merely 'permissible'. Yet as my account of McLaren's use of PGD in her pro-research lobbying shows, the view that research is obligatory is itself also expressed in idea of 'pre-embryo' as a usable tool: the distinction in development explains how McLaren in 1978 managed to reframe genetic malformation as a reason to fear research into a reason to support it. As the debates moved forward, the 'pre-embryo' was central to the argument for intervention, as it became

connected to a new set of values expressed in the idea of clinical application, of which the social benefit of healthy babies was one; but there were also others. The relationship between the arguments for the 'pre-embryo' and for PGD in the pro-research case represents more than a matter of nuance because, as we will see, it is precisely through this relationship that the public view of artificial intervention in biology changed permanently. A relativistic understanding of biology in which artificial intervention, especially in the genetic sense, provides a blueprint for a drastic and lasting shift in public attitudes towards reproductive technologies has lasted well beyond the conclusion of the 1980s debate.

The description of the process by which the biological model and moral values came to be related thus has implications for the way that morality, too, is understood. Mulkay argues that the 'pre-embryo' "allowed the pro-lobby to apply the same moral principles that prioritised the rights of the individual to the question of embryo research, thereby reconciling the idea of human embryo research with "generally agreed moral principles" (Mulkay, 1994, p. 624). My account suggests that human embryo research was not merely aligned with pre-existing moral principles, but that this set of morals was negotiated iteratively through the process of superimposing visions. The outcome of this process is this linking of the 'pre-embryo' to PGD: an overdetermined common ground was established through a vision of assisted biology in which research and application were linked towards a shared moral goal.

The case of PGD here given also offers an example of what Franklin (2013a), drawing on Marx, has called 'substantialisation', which expresses the relationship between human 'embodiment, sociality, identity, material objects, and technology' to show how humans co-evolve with their environment, so that a concept is substantialized through technique, not just inherited equipment. In this understanding, tools are more than equipment, but also expressive of and influencing ontologies and epistemologies,

> Tools, and the evolution of technology, must be understood as both inherited equipment and as the moulding conditions of human existence, constantly reshaping what the human is by what it can do, in a dialectical process that extends beyond historical time into the mists of human species emergence. More than this, tools are never merely instrumental: as Heidegger insisted, they belong to the history of thought, and as Marx also argued, tools are the offspring of imagined worlds as much as actual ones. Tools are substantialized concepts.
>
> Franklin, 2013a, p. 13

In this sense the tool can also exceed what is currently known. Indeed, Franklin

describes “both science and reproduction as frontiers that are shaped by open-ended exploration largely based on the use of handmade and hand-held tools.” (p. 106). The reworking of the biological in dialectical relation to PGD as it was being developed, shows how this model in which social and scientific technology and concept are intimately related, can also be applied to the negotiation of moral values. Here the technology of PGD substantializes a commonly held biological model by incorporating not only new scientific information, but also new concerns, feelings, and values.

This insight is also palpable in the literature on postgenomic biotechnologies in Science and Technology Studies (e.g. Mackenzie et al., 2013). Here, scholars have argued that new biotechnologies can be studied in terms of how they become part of a process of arriving at new standardised definitions of the ‘biological’ (ibid). In so doing, these technologies “amplify the regimes of engagement” and “explicitly or inadvertently invoke, for instance, publics who can add something that the scientists or engineers cannot” (Mackenzie et al., 2013, p. 704), as their biological standards are negotiated in direct relation to an imagined set of users. In this way, standardized technoscience is “co-imagined with standardized publics” (Ellis, Waterton, and Wynne, 2009) – these biological tools shape what is biologically possible at the same time that they become “vectors of tangled values, beliefs and desires concerning speed, control, and economy” (Mackenzie et al., 2013, p. 716). It is crucial, these authors claim, that we recognise that “in finding a way of pledging material arrangements to a sought-after good, there are risks and responsibilities involved in creating standards” (ibid). McLaren’s case shows us how one scientist negotiated this responsibility.

Biological reasoning

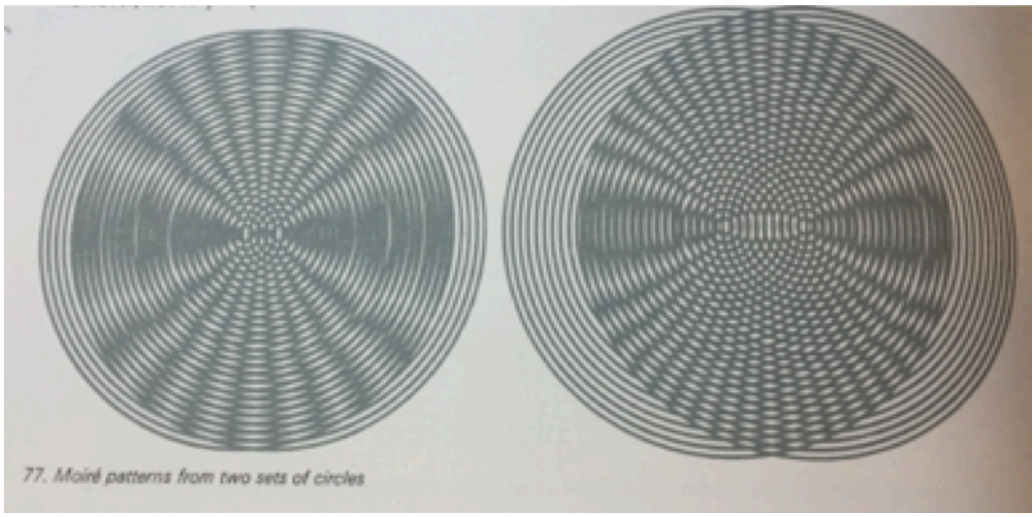

77. Moiré patterns from two sets of circles

Figure 5. Moiré Patterns, *Behind Appearances*, 1969, C.H. Waddington, p. 124. In this diagram, Waddington demonstrates how by overlaying two patterns, he creates a model system to study these patterns in relation to one another, addressing the problem of organisation that he claims is of central interest to the new, 'Third Science'.

The embryologist and polymath C.H. Waddington wrote a book titled, *Behind Appearances* (1969). In it, he outlines a shift in scientific thinking in the 20th century, evident in the theories of evolution and relativity, that no longer conceives of the world as "consisting of solid lumps of matter which can be organised into straightforward machines" (p. 1). He outlines a movement from the science of the Ancient Greeks, where axioms would be proposed from which "theorems could be deduced by the application of logical systems of inference", to a Second Science associated with the Renaissance based on the interrogation of nature through experimentation and whose questions are formulated in terms of entities such as atoms, forces, masses, to a Third Science, sparked by theories of relativity in physics, or evolution in biology, that acknowledge indeterminacy and chance as "fundamental characteristics of reality", as well as the technical means, with the introduction of computers, to analyse systems of organisation (p. 2), causing a shift "away from atomistic, decontextualised science", into the study of "such general properties as information or organisation. These objects of study are not entities in the usual sense but are characteristics of systems" (pp. 2–3).

Waddington also tries to relate these shifts in science to shifts in the history of art, with a focus on painting. Here, too, he notes how artists no longer look for likeness of delineated entities, moving away from the 'representational'. He brings his analysis of art and science together by drawing on Thomas Kuhn's model of the paradigm shift in *The Structure of Scientific Revolutions* (1962). In Kuhn's theory, he explains, each era sees the world in terms of a particular 'paradigm', and this paradigm comprises both "an apparatus of perception (which brings into particular focus certain aspects of our existence) and a framework by which the many different facets of the universe can be related to each other" (Waddington, 1969, p. 1). Perception, then, reflects a certain way of knowing. He goes on, "our general notions about the world – the spectacles through which we see it and the framework into which we try to fit our observations are, of course, dependent in the main on what we know about it" (ibid). And so, by drawing on Kuhn to outline a shift in world view over the first half of the 20th century that is much broader than that within the particular scientific disciplines that Kuhn addresses, Waddington describes a new scientific approach.

Particularly the new concern with organisation, he asserts, has profound implications for attitudes to the structure of science more generally, or the ethos of doing science. One consequence is a recognition of the multiplicity of the sciences, the idiosyncrasy of their perspectives. The study of organisation means studying the ways in which units are related, often "the resulting complex may exhibit properties which cannot be expressed by the objects in isolation" which means that "a science which deals with properties of one type of organised entity may, and probably will, be very different in character from that which deals with different kind of organisation" (p. 127). It would be difficult, he goes on, "ever to comprehend exhaustively the entire content of any class of natural phenomena" and so "we cannot hope that the different sciences will, in general, merge with one another again" (ibid). Each science, in this way, has a "particular selection of methods, an intellectual pattern appropriate to the behaviour of its subject matter" (ibid.).

Waddington, then, describes how each science represents a partial perspective characterised by a set of specific questions. This view resonates once again with Strathern's (1992b) concept of 'merographic thinking', in which "partial connection from another angle" can displace one meaning for another – a process she describes as substitution in her critique of the metaphysics of nature and culture" (p. 304). The relational epistemology leads to an understanding of individual sciences as partial, relative to the question and the scientist performing the inquiry, and this also informs Waddington's interpretation of the images produced within science. The Third Science, he claims, is accompanied by a new aesthetic that expresses this interest in questions about organisation by recording "the interaction of two or more intersecting patterns" (Waddington, 1969, p. 122). The simplest example of this new type of image is the moiré pattern (see figure 5), which "can be produced by superimposing two systems of concentric circles on each other" which Waddington offers as an example of the kinds of problems the Third Science is interested in: namely, "a system that is itself produced by the interaction of two subsystems" (p. 124). As in his description of science as a whole, questions are conceptualised as a set of relations that can only ever be appreciated in part, at particular nodes, and as part of a particular system.

The pattern and scientific ethos underlying Waddington's conceptualisation of the Third Science and its new aesthetic connects to the concept of 'framing' I have used to describe McLaren's contributions to the HF&E debates and resonates in interested ways with the approach she took to finding common ground. As we have seen, her process of

incorporating public views into her Warnock case was a process of bringing two distinct views of development – one teleological and one contingent – expressing different priorities and agendas, together into a scientific picture that showed how they overlapped under a common aim of clinical translation. As we saw in the previous chapter, too, and here in the case of PGD, this process requires feeling as well as rational thought in order to distil the concerns represented by any position, to understand how the account is framed in relation to specific concerns. McLaren thus displays an awareness of the partial framing of any account in relation to its context and purpose and uses this insight to find meaningful common ground through translation or superimposition. The clinical or the research perspective are both equally true, but both offer only part of a bigger picture. In this way, McLaren's case for research described a set of relations – between basic and clinical science, law and science, between scientist and potential patient, and their respective morals and feelings. The model therefore positions the scientist not as an arbiter of definitive facts, but as a figure bringing clarity to the discussion by making explicit the framing of respective positions and their limitations and intersections. In this sense, she practices the newfound self-reflexivity that Waddington attributes to scientists of the Third Science, witnessed in the breakdown of the old distinction between the observer and the observed:

> The scientist himself, or man in general in his activity as an observer, comes to be incorporated into science in a way which is completely outside the Second Science paradigm. And finally, he also becomes involved in science not only as a maker of it but as a subject for its study, not only as an individual with a psychology, but as a member of a set of societies which are themselves examples of organisation. Third Science, which is still nascent and has not yet arrived at a definite inclusive paradigm of its own, will certainly have the human and social science as very important factors in its make-up.
>
> Waddington, *Behind Appearances,* 1969, pp. 2–3

The above quote shows how Waddington's conception of the new science implies a new role for the scientist, as aware of the limitations of any particular discipline, and therefore more amenable to inter-disciplinary collaboration. This type of thinking is very much evident in McLaren's policy work and, we will see in the next chapter, also in her approach to laboratory science. As with Waddington, there is a consistent understanding of the role of framing, of perception in the production of knowledge, and this awareness arms her with a strategic toolkit that allows her to solve problems, also within science, in a non-traditional but highly effective way.

Waddington describes a general trend across the sciences, but he labours the point that biological thinking in particular was already adept to the new conceptualisation of life in terms of organisation. Biologists were starting to see just how "universal organisation is" – "in day to day physiological functioning, in developments from egg to adult, in long-term processes of evolution" and science more generally was beginning to follow suit, although under "somewhat different names" such as "'feedback control', 'end-product inhibition', 'error-correcting networks', and the like – very similar ideas have been growing in importance in the physical as well as the biological sciences" (1969, p. 118).

In the next chapter I will turn to a fuller explanation of the concern with process and organisation among the so-called 'organicist' biologists, and how McLaren can be placed in this lineage, but here I point to the way in which biological thinking already lends itself to thinking through problems in terms of relations, and of conceptualising and, therefore, visualising its object of study in these terms. Evelyn Fox-Keller has described this distinctive biological way of seeing as the 'biological gaze' (1996) and offers a range of examples in the history of the life sciences in which the tools used to make biological objects at the start of life visible also unavoidably change them, thereby complicating the notion of objective observation, so that "what we see as we gaze at the secret of life is life already, and necessarily, transformed by the very technology of our gaze. And conversely, and simultaneously, that gaze provides the means of further transformation" (p. 120).

Especially at the microscopic level in modern biotechnology, in the fundamental processes of generation, it becomes clear how there is inherent to biology the process of preparation and modelling in order to bring to light the specific problem the biologist is interested in. IVF, as Sarah Franklin (2013a) has argued, goes even further by showing not only "how socialised (and socialising) scientific understandings always are, but now also, and ever more visibly, how social values, systems, and aspirations are being engineered and constructed in such a manner that they too become part of what 'biological' means" (p. 70). As Keller's quote above also illustrates, the process of intervention is both disruptive and enabling. Donna Haraway has conveyed a similar lesson in, among others, her monograph *Crystals, Fabrics and Fields* (1976), where she follows Thomas Kuhn and his notion of the paradigm as also discussed by Waddington, to show how 'metaphor', and in her later work, the 'model', "is the vital spirit of the paradigm (or perhaps its basic organising relation)" (p. 9). These metaphors "embody expectations and fundamental views of a structure of nature" making scientific theories testable while they also "lead to a

searching for limits of the metaphoric system and thus generates the anomalies important in paradigm change" (1976, p. 8). The biologist, in this way, is adept in the back and forth movement between metaphor and referent through "the application of the analogy" (p. 10). This biological gaze begins to suggest how McLaren was primed for the kind of thinking that distilled diverse perspectives into workable models – models that change as the technologies of science and of society in the form of the law and public attitudes transgress the boundaries of what is known.

McLaren proved adept at superimposing these perspectives in order to build a common ground expressed in a biological model that, as we have seen, was very different from any of the individual perspectives incorporated. Similarly, in the accounts of Waddington, Keller and Haraway, the biologist is an expert at the work of framing the question into enabling models that foreground a set of relationships comprising a system. In the next chapter, I will offer some examples of how McLaren engaged this kind of thinking in her science, and how this resonates with her approach in policy-making. Here already, though, I want to note the resonances between the style of thought required to bring different perspectives together in developing the Warnock model, of describing the policy question through a process of framing, and the concern in biology with organisation studied through models that make visible the phenomenon at hand.

Genetic thinking

As we have just seen, PGD, which emerged as a result of this early framing by McLaren that posited the need for research based on prospective clinical translations, was also formed to reflect public concerns, so that a research agenda prioritised developing techniques that would select, rather than alter, the embryonic genome. In addition to changing public understandings of development implicitly, the emphasis on genetics as a means to clinical certainty also showed in concrete and scientific terms the limits of genetics in explaining development, a curious legacy considering the role that genetics have continued to play as a source of hope in the debates on therapeutic cloning and stem cells since the HF&E debates. The obstacles encountered on the path to achieving PGD revealed the limitations of genetic analysis in the securing of a healthy birth, thereby validating the embryologist's vision of contingent development over the teleological one. In a 1989 Ellison-Cliffe lecture, *Research on the human conceptus and its regulation in Britain today*, which discussed possibilities for genetic analysis of gene products in the embryo, McLaren

writes,

> But this raises another problem, which would never have been uncovered without IVF research, and which again emphasises the risks of extrapolating from mouse to human … In the human … it is not until the 4–8 cell stage that the embryonic genes start to be expressed. This means that any attempt to do pre-implantation diagnosis at the eight-cell stage in the human by looking at gene products would be in danger of diagnosing the mother's genes instead of those of the conceptus.
>
> McLaren, 1989, pp. 211–12, unarchived document

This discovery of the late activation of the embryonic genome also explains, McLaren goes on to show, the need for the polymerase chain reaction (PCR) that can be used to amplify a particular stretch of DNA so that the DNA itself, rather than the gene product can be analysed. The case shows however, that on the path to developing means of genetic diagnosis in aid of embryo selection in order to maximise the possibilities of certainty in pregnancy, scientists gained a refined insight into the workings of the embryonic genome in early development that complicated the developmental picture and contradicted any simplified, deterministic view of development as the expression of one narrow conception of 'potential' usually represented as the expression of genes in popular debate. By responding to patient priorities and taking their developmental picture into account, in the form of a clinical aim, the biological model of development as contingent was further validated. This gives another sense in which the biological picture used in the Warnock debates became increasingly justifiable in 'scientific' terms, as PGD was realised. The sociological and legal rationale for a cut-off point in development became less necessary, as the use of the term 'pre-embryo' in clinical science increasingly spoke for itself. The view of biology expressed in these arguments for research based on the possibility of PGD is changed. As Franklin writes,

> … greater proximity to genetic information may be experienced as increasing its uncertainty as well as its fixity or determinism (see in particular Strathern 1999). This tendency to dichotomise these possibilities (either DNA "tells you who you are" or it doesn't) may thus be an analytic tendency at odds with a growing amount of empirical data (see esp. Lock 2005).
>
> Franklin, 2006, p. 223

In McLaren's biological model for development, and reflected in her case for embryo research overall, at least, we see that genes and the determinism that they might imply in popular accounts of development are only part of a layered account that contains other biological criteria (physiological coherence, sentience), as well as social, clinical,

legal and ethical considerations. The biological model thus comes to foreground the contingency of any relationship between embryo and person, thereby translating a new biological view, as well as scientific research, into applied therapies and into the debates.

This new biological vision, then, is a crucial legacy of PGD still noticeable in the public debate today. Unlike the other analyses of the debates that we have seen that separate the biological case for the 'pre-embryo' from the role of PGD in explaining the outcome of the debate (Gunning and English, 1993; Jasanoff, 2005; Mulkay, 1997), I have shown that this legacy can only be explained by understanding the relationship between the arguments for clinical translation and the biological model used. Bioethicists Jennifer Gunning and Veronica English (1993) try to explain this legacy of a new biological vision and draw a connection to clinical applications centred around genetic screening to explain how the debates saw a widespread shift in understanding of the biological facts of development. In their comparison between the legislative processes in the UK and the US, they show how clinical application was distinctively central to the UK case from the beginning of the policy debate. By framing IVF, initially still an experimental procedure, as a therapy in the Warnock Report, the authors claim that the focus of deliberation was shifted from the controversial terrain of research towards the less divisive one of therapies. Moreover, they argue that an emphasis on genetic possibilities also changed what was considered therapeutic research in the first place. Now it was not just the alleviation of infertility but also the prevention of genetic disease that required interventions at the molecular level, bringing laboratory and clinic ever closer together in the public view. The arguments for research in aid of PGD in this way paved the way for the acceptability and desirability of basic research in embryology and inherited disease generally, of using "human embryos, animal models, but also inter and intraspecific IVF, to provide information on genetic defects" (Gunning and English, 1993, p. 24).

Genetics, then, with its implications of linearity and determinism, has also served to recruit support for a wide range of research projects into the contingency of development. The rise of regenerative medicine and its dependency on models of development that are temporally malleable, are testament to this paradoxical legacy – and it is the consequence of superimposition, of a set of imperfect translations that never fully paper over the cracks. I hope, then, in this chapter to have demonstrated Gunning and English's point with a description of how this legacy of the acceptability of genetic research was cultivated through a biological model that connected the certainty offered by genetic screening to

contingent and continuous development, substantiating a close relationship between laboratory research and clinical application that came to be broadly shared.

Translational robustness

I have described the legacy of 'genetic thinking' emerging from the model of superimpositions employed in the HF&E debates in order to develop and further schematise the model of translation that I am proposing. Through the account of McLaren's contributions to the Warnock debate, I have suggested an alternative evaluation of the 'pre-embryo', not as a 'fudge' nor as a rhetorical tool, but as part of a translational model aimed towards finding consensus through public deliberation. I have described the use of the 'pre-embryo' and the biological vision that it reflects, as a layering of different perspectives that are representative of different groups and different epistemologies. What emerges, as we have seen, is a picture that always remains complex, that always reveals the cracks in its foundations in the form of confusion, discomfort and disagreement among the various parties involved. Interestingly, though, despite the non-literal translation of the scientific vision into a linear account reflecting the certainty that genetic screening could provide, the scientific vision of a totipotent, continuous biological cycle has been validated in the public eye and provided a widespread acceptance of, and even support for, intervention in the biological cycle. While both the teleological embryo that gives rise to a baby and the totipotent embryo that is used in the lab continue to coexist, the biologist's and patient's vision of development has moved closer together. As the ongoing support for the HF&E Act and its amendments to accommodate new clinical possibilities suggests, this model and this vision have been remarkably robust – despite the scientific community's apparent disdain for the term 'pre-embryo' and despite the fact that the term is no longer used, the vision that it connotes remains intact.

This brings us back to Strathern and her theorization of knowledge practices and how they stand alone or intersect according to their context. In her book *Reproducing the Future* (1992b), Strathern draws one of many analogies between language and the embryo: "like developing cells," she says, "language has a constant potential for increasing differentiation" (p. 143). She explains this characteristic by placing emphasis on the environment, the context of ideas:

> In cultural life, in those habits of thought about which for most of the time we are very much unaware, the ideas that reproduce themselves in our communications never reproduce themselves exactly. They are always found in environments or

> contexts that have their own properties or characteristics. These environments or contexts provide a range of domains. We can think of all the social differences that opportunity, class, gender, expertise and so forth make to how the world is perceived; interests such as these form several such environments, and profoundly shape the nature of communication. Moreover, insofar as each is a domain, each imposes its own logic of 'natural' association. Natural association means that ideas are always enunciated in an environment of other ideas, in contexts already occupied by other thoughts and images. Finding a place for new thoughts becomes an act of displacement.
>
> Strathern, 1992b, p. 6

If then, a 'new' idea is only ever articulated in a context already representing a node in a web of pre-existing ideas, it can never be reproduced exactly because every context is different. Given this understanding, non-literal translation is the only way to preserve meaning, and non-exact reproduction is the only means to survival. Like the embryo, the robustness of meaning comes from an ability to adapt to an environment with its own set of 'properties and characteristics'. This points to the aptness of the model of superimposition. Again, this is a geological idiom that does not mask the process of the compression of layers upon which it relies. Consensus is built on shifting ground and it is in these tectonic movements that the vitality of meaning resides. Disagreement does not preclude consensus, but instead describes the unavoidable context in which any, always temporary, legislative outcome is found. The legacy of the biological model used in the HF&E debates reveals how it was the imperfect reproduction of the biologist's model that allowed its underlying meaning to survive, indeed, to thrive in the form of an expanding and proliferating research agenda, which in turn, in a recursive loop, came to confirm the biologist's view. By adapting the model to the values, feelings, legal requirements, and ethical ideas that defined the context of the HF&E discussions, the scientific model of development was kept very much alive.

Legacy of the biological model

McLaren, as is clear in description of development in *Where to Draw the Line*, was invested in a model of development that was continuous and contingent. She also, however, believed that clinical applications should always be discussed in relation to research and vice versa. This latter conviction allowed her to present the scientist's view in a public debate, moulding her biological picture to embody shared moral concerns. McLaren was thus anything but opposed to emphasising the possibilities of genetic diagnosis, and the certainty that this could bring couples, despite the fact that this offered only one perspective on a

more complex biological picture. As Professor Marcus Pembrey (Pembrey, interview with author, 17 October 2018), remembers, McLaren recognised from early on the role that genetics would come to play in the public debates over embryo research. She also encouraged him to join Progress and later to chair Progress Educational Trust. He recalls,

> And very interestingly, she said, "You're exactly the right person to do that because the next big issues is not going to be embryo research, it's going to be genetics. What genetics can do, and so on, and all the ethics of that". And she also knew that I, in '88, I'd started working with Gene Goulding to set up the Avon Longitudinal Study of Parents and Children, where we would be doing DNA analysis on the general population for the first time. It would be the first cohort ever where we would be collecting DNA and start analysing it. So she basically encouraged me in all of this.
>
> Pembrey, interview with author, 17 October 2018

McLaren actively encouraged research in this area while also encouraging the leaders of these research agendas to guide public discussion. It is perhaps also with an eye to the future, then, that she was cultivating a role for the scientist in public debate in relation to PGD that revolved around a new model for scientific intervention in development. This biological vision, this shared moral ground, has in many ways persisted through the debates on embryo research that have arisen since the passing of the HF&E Act. The response to the proposed changes to the HF&E Act in 2008, are one gage of how attitudes have permanently changed. The proposed HF&E Act II was to accommodate "the formation of admixed human embryos for research, using the eggs of cows combined with human nuclei to create new biological tools" (Franklin 2014a, p. 118). Sarah Franklin reflects on the protests and reactions to the proposed changes to the Act:

> However, the reduced size and fervour of the demonstrations for and against embryo research were not the only measure of the difference between 1990 and 2008. Looking back, we can see that although some forms of reproductive technology – such as cloning, human-animal hybrids, and stem cells – still engender controversy, the logic of progress associated with assisted conception has been sedimented into a naturalized trajectory of intervention in the name of human betterment in which technological manipulation of human embryos is not only a viable alternative to 'natural' reproduction, but also a necessary path to the continued improvement of human health.
>
> Franklin, 2014a, p. 120

Unlike the 1980s debates, there was now a strong majority in favour of the mixed animal-human embryos, reflecting the consolidation of a moral position based on a biological vision of assisted biology that impels us to intervene in the name of human

betterment. As Strathern (1992a) has described, the process of 'literalisation', or of making explicit values in relation to the facts-of-life in order to generate new ones, means "there is no going back", that one "cannot recapture the point before explicitness" leading to a perpetual "relativizing of 'our' understanding of 'ourselves'" that produces the sense that there appears to be less and less to be taken for granted and thus less nature in the world" (p. 44). Franklin echoes this insight specifically in relation to the legacy of IVF in Britain:

> Over time, the connection between IVF and fertility – or even conception – has been superseded (as it was preceded) by a more general isomorphism between improvements to human life and the ability to culture human embryos in glass. This is the biological relation IVF substantializes as both a model system and an ethical consensus, and therefore not only as a translational path but as a public duty. This is the logic of remaking life that appears increasingly to have become a sign of a vital, caring, and creative Britain.
>
> Franklin, 2013a, p. 61

PGD, as we have seen, built on this sense and consolidated this new British ethos based on 'ethical consensus'. This increasing sense that biology was, and should be, assisted was, in turn, assisted by the carrying forward of the Warnock model, which expressed the sense that biology is relative not only to technological intervention, but also to clinical horizons, the state of science, regulatory provision, and public consensus. While the term, 'pre-embryo' may not have survived in public or medical debate beyond the 1980s, its translational logic demonstrably has.

In the Chapter 4, I will discuss how this translational logic might assist us in contemporary debates about the possible extension of the 14-day rule, in light of an arguably changed biological vision in which assistance has reached new heights in the form of synthetic biology – where the concept of natural development, when an embryo exists as a delineated object, is obsolete. How, in this case, might we think about drawing lines as a process of finding common ground instead by drawing on various disciplinary logics and the concerns of various affected groups?

Conclusion: McLaren's problem-solving ethos

In the previous two chapters, we have seen how McLaren, in the context of the policy debates on human embryo research, actively orchestrated and consolidated a structure, a specific framing of the discussions taking place in scientific meetings, committees, in Parliament, and by special interest groups. This work of framing shows how, to McLaren,

her work as a scientist consisted of much more than bench science. PGD provides a case in point. In addition to the encouragement she gave to eminent scientists in the field of reproductive biology such as Marcus Pembrey and Robert Winston to join lobbying groups like Progress, and to Marylin Monk in developing the scientific techniques required, she also groomed the social and political ground by developing a scientific and ethical model that would facilitate research. As in the previous chapter, we saw how McLaren brought together disparate domains in her translational model, which defined her distinctive style of practice at the same time that it renders her legacy sometimes ambivalent as scientists grasp for the language to describe her approach. Indeed, I have shown the rise of a more relativistic biology over the course of the debates, and have tied this to McLaren's expertise, showing how she combined authoritative scientific understanding with the idea of moral purpose and social good, necessitating a language that could reflect both. This description of the scientist's role in public debate again models the translation I am proposing, one that begins with an understanding of the biological facts as relational, to move towards interpretations of the facts that are representative of the ideas and values of a broader social identity.

It is nonetheless the persistent difficulty in describing this model that also explains McLaren's own need for new terminology, including but not limited to the 'pre-embryo' and her model used in the HF&E debates and expressed in the various speeches and diagrams she used, as she tried to bring values, facts, feelings and hopes together to express her sense, as a scientist, of what was right. The legacy of this distinctive role, I have also suggested, is that we think increasingly in this way, even in the absence of terminology to describe the process.

In the next chapter, I will place McLaren's approach as I have described it thus far in a broader description of McLaren's ethos in science, and her developing thinking about the relationships between science and society that emanate from this conceptualisation of her own role as a scientist. Her career comes to represent, I will show, a testament to the importance of framing to any debate. Her contribution in all cases is one of a distinctive style of reasoning, a style that falls into a legacy of biological reasoning characterised by socialist scientists in Britain in the 1930s, and one that is particularly relevant to contemporary discussions about biotechnology.

Chapter 3

McLaren's style of practice

Over the course of the debates on human embryo research in the 1980s, McLaren developed a distinctive role in which she combined an authoritative scientific understanding with the idea of moral purpose and social good. This unusual position necessitated a new language that could capture these disparate perspectives, a language that she found in the form of a set of superimposed logics. McLaren's contributions show how the scientific facts were translated into the discussions not literally, but were adapted to the context in order to accommodate the broad range of concerns that were linked to the public debates on human embryo research. In this way, however, the biological vision from which she departed remained curiously intact, as is evident in the more relativistic model of biology that is assumed by a public generally supportive of a wide range of experimentation in the field of genetics and reproductive biology today (Franklin, 2014a).

How did McLaren come to cultivate this approach? How can we describe her approach to public debate by referring more broadly to her formation as a scientist? What elements of her biography help to illuminate her distinctive style of practice? I will address these questions by describing in this chapter McLaren's political and scientific background beyond her role in the debates on human embryo research in Britain. By looking at McLaren's educational and personal biography, describing her academic and political lineage in relation to public engagement with science, in addition to her own writing on the relation between science and society, I attempt to describe broadly McLaren's overarching social vision, which I describe as an ethos of problem solving.

The language used by my interviewees to describe McLaren's approach as a scientist resonated and can be categorised under three broad traits. The first emphasised by many scientists, including Peter Goodfellow, Richard Gardner and David Whittingham, was McLaren's ability to connect researchers: she was, as Goodfellow (interview with author, 21 September 2018) puts it, 'everybody's aunt'. We have already witnessed this ability in the account of the human embryo debate, where she was able to unite researchers in order to move a translational agenda forward, for example. The second quality repeatedly emphasised, was McLaren's clarity of vision, most notably in Warnock's recollection of her role in structuring and framing the questions at hand on her Committee. Finally, former

collaborators repeatedly emphasise her breadth of vision; her ability to look across scientific disciplines, and to broader social, ethical and political questions in order to move scientific agendas forward (interviews with author, 2018). Her role in the debates on human embryo research again provide a case in point, as I have described by reference to the series of translations McLaren performed across domains in order to make a case for research. These three capabilities were, moreover, as we have already seen, all mutually instantiating. McLaren's breadth of vision was, for example, what gave her clarity on the Warnock Inquiry, and allowed her to frame the question at hand as a shared research agenda that then allowed her to exercise her ability to connect individuals under a common goal. These traits reappear and converge in McLaren's other pursuits, from her scientific to her policy work, generating a distinctive pattern of practice, as McLaren moves between social circles to recruit and connect new perspectives and people to her overarching scientific goal.

By describing here again how McLaren's approach manifested, we begin to see how her idiosyncratic style was defined by a particular social vision – she drew on her ability to translate in all the senses we have seen to work towards her understanding of a better, fairer, world. Describing McLaren's social vision more broadly also helps distil the social implications of the model of public deliberation in the context of the debates in the 1980s.

Geological idioms: Waddington's landscape

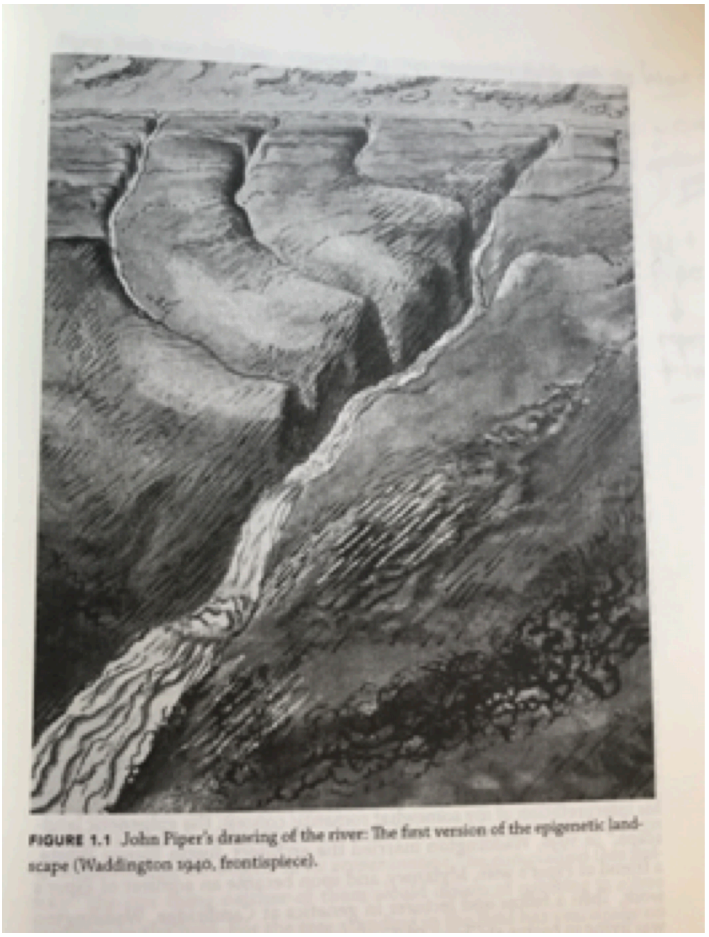


FIGURE 1.1 John Piper's drawing of the river: The first version of the epigenetic landscape (Waddington 1940, frontispiece).

Figure 6. Image of Waddington's 'epigenetic landscape', a metaphor for development, as painted by John Piper, *Epigenetic Landscape*s, Squier, 2017, p. 23.

As we have seen, McLaren's distinctive approach to the policy discussions on human embryo research in the 1980s was characterised by her combining authoritative scientific rationale with the idea of moral purpose and social good. McLaren's thinking in science, too, resonates with the most creative ways of modelling development that have emerged in the field. The cultural critic Susan Squier (2017), for example, has detailed how the embryologist C.H. Waddington (1905-1975) used his model of the so-called 'epigenetic landscape' to propose a very different way of conceptualising the problem faced by the embryologist in the 1960s as molecular biology and embryology began to unite.

Waddington belonged to a group of so-called 'organicists'. These were socialists and scientists who were committed to conceptualising a new model for the organism as it develops from genotype to phenotype. Waddington in particular expressed hopes that this model would incorporate a growing understanding of biological development as an "interactive and recursive set of systems" (Squier, 2017, p. 44), that could extend beyond

the organism to its – perhaps even, social, as Squier shows – environment. The epigenetic landscape was the "visual image developed by Waddington as the central figure for the [new] scientific field of epigenetics, the "causal analysis of development" initially intended to facilitate greater collaboration between molecular biology and embryologists by framing development in such a way as to represent a shared problem (Waddington, 1940, cited in Squier, 2017, p. 2). For Waddington, then, the challenge in producing the image was epistemological: he wanted to find a way to link embryological development to hereditary transmission through the representation of temporal process in visual form. Waddington's landscape accordingly modelled a hypothetical relationship between two domains and invited practitioners from various disciplines to 'fill it in'. The image was not intended as a realistic sequencing of embryonic development as was conventional in the field (Hopwood 1999; Squier, 2017), but served as a visual metaphor for the process of development, intended to build a bridge between embryology and genetics. The landscape, in this sense, expresses a largely unknown system; it is an open question. The scientist, in this model, intervenes in this system which they "believe has certain stability characteristics" but Waddington explains, "we have no idea where we are on the landscape when we first start trying to affect the system" (Waddington cited in Squier, 2017, p. 12). The landscape is too big to ever traverse in its entirety and Waddington therefore prescribes a rough method for exploring the landscape in which the scientist "alternates between (a) local exploration and (b) a jump in the dark to try to change some quite different aspect of the system" (Waddington 1977, cited in Squier, 2017, p. 12). This way of conceptualising the scientist's relation to their object of study is quite different from the narrow expertise of the more conventional scientist. Here, the scientist attempts to understand a system, not an object, and this system is only ever a subset of a large network of interconnected systems that they can never fully perceive. The scientist is limited by their position in the 'landscape' which constitutes the particular part of the hypothetical model, the subset of related systems, that they choose to explore.

Squier goes on to draw on the philosophy of Michel Serres, vis-à-vis STS scholar Bruno Latour, to characterise the epistemological approach Waddington posits in his instructions for 'exploring a landscape'. Latour describes Serres' proposed mode of learning, called '*tiers-instruit*', to explain how this differs from "the standard Western epistemological model" of critique and subordination to a single epistemological category (Latour, 1987; Serres and Latour, 1995 in Squier, 2017, p. 13). Serres' mode of learning

relies instead on a "wandering, translational commentary that ranges across disciplines and disciplinary languages" (Squier, 2017, p. 13).

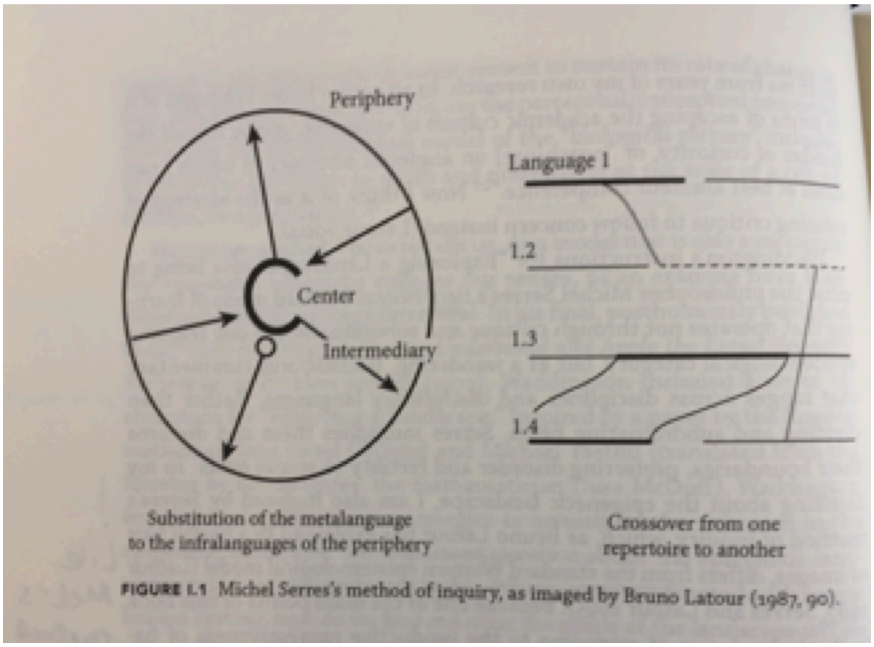


Figure 7. Diagram from Epigenetic Landscapes, Susan Squier, 2017, p. 14. Squier replicates a diagram created by Bruno Latour, in which he shows the difference between philosopher Michel Serres' method of inquiry and that of the traditional 'critique philosophers'.

Serres' approach, represented by the second diagram of layered languages is contrasted to the one summarised in the first image shown in figure 7. Here, a circle marked by arrows extending outward toward the periphery and inward to the centre "represents a powerful critique … that ties, like a bicycle wheel, every point of a periphery to one term of the centre through the intermediary proxy" (Latour, 1987, p. 90 cited in Squier, 2017, p. 13). This mode represents, Latour explains, the "Critique philosophers," who "firmly install their metalanguage in the centre and slowly substitute their arguments to every single object of the periphery" (ibid). Serres' "pre-critical philosophy" is, in contrast, expressed as a series of "parallel lines stacked one on top of the other, with the tiered labels 'Language 1, 1.2, 1.3., and 1.4.' and wavering and straight lines linking the tiers" (p.13). "Crossover from one repertoire to another," the caption reads. Latour defines Serres' method not as critique but as commentary, a "cross-over, in the genetic sense, whereby characters of one language are crossed with attributes of another origin" (Latour, 1987, p. 90–91, cited in Squier, p. 13). For Squier, the implications of this crossing-over extend beyond disciplinary crossings within science.

Waddington's epigenetic landscape, for Squier, has the potential to construct new epistemologies that draw on extra-scientific discourses and frame development across new levels of analysis. Squier reads the epigenetic landscape into contemporary examples of landscape architecture and bio art to show how the landscape's alternative conceptualisation of the object of science as a problem allows it to serve as a methodological tool for remediating development. In particular, the landscape captures different layers of time that allow broader, 'environmental' factors to be factored into accounts of development, which can come to encompass a whole "ecology" of social factors beyond those in molecular or cellular biology alone (Squier, 2017, p. 183).

Scientific inquiry is not only exploratory but also, importantly, a process of allowing oneself to be affected, and one's languages to be inflected, by the languages of other domains. Rather than "dividing and subordinating fields, Serres multiplies them and disturbs their boundaries, preferring disorder and fertility to sterile order" (Squier, p. 13). Here we return to the imagery of superimpositions that Waddington also described in *Behind Appearances* (1969), using the moiré pattern – both the landscape and his exploration of the relations between science and art can be seen as part of a search for adequate visual representations of the problems in science and, in both cases, the answer seems to be layering logics: exposing relationships, understanding contextually, relationally, and translationally in the sense that I have described throughout this thesis. Serres' conceptualisation helps to tease out McLaren and Waddington's shared epistemological assumptions. Serres' methodology also resonates with McLaren's approach of superimposing in arriving at consensus, expressed in a model of layered logics in which she allowed her scientific language to be changed by the context of public debate. Like Waddington, McLaren was explicitly exploring ways of modelling the set of relationships in which she was interested: between the domains of science, ethics, law, and feeling, for example.

The 'pre-embryo' came to demarcate first a research agenda geared towards clinical application, and then increasingly a biological picture that reflected the way that this clinical translation worked. The picture was shaped in a very real sense by the concerns of a general public around genetic defects and the desire to have healthy babies. McLaren, like Waddington, invoked a geological idiom with her concept of superimpositions to describe and develop her understanding of her role in relation to her object of study. This relationship, as we have seen, was relative to a particular agenda; partial in the sense that

it was framed by a specific goal towards clinical application. McLaren's policy work thus already shows how the legacy of Waddington's landscape, like Squier argues, is in the way its logic has been used to model relationships that extend far beyond embryology and genetics – it is, in the multiple sense that I have described in this thesis, translational.

It is also possible, however, to trace the influence of Waddington's thinking on McLaren's way of approaching laboratory science specifically. Indeed, McLaren in an interview for the British Library Mammalian Developmental Biology archive in 2007, cites Waddington as one of the main scientific influences on her work (interview with Franklin and Johnson, 2007), and worked at his laboratory in the Institute of Animal Genetics in Edinburgh for fifteen years between 1959 and 1974. Moreover, both Waddington and McLaren were Marxists, and educated through the Communist Party.

In her science, McLaren shared Waddington's commitment to a view that might be described as 'epigenetic', and, like Waddington, she found herself wanting for a field that captured the scope of her research interests. There did emerge a field that came close to what we will see was McLaren's integrated understanding of reproduction, genetics and developmental biology, in the late 1970s, under the bracket of 'evo-devo', whose followers aimed to link development to inheritance. Steven Rose, neuroscientist, social commentator, author, and a friend of McLaren's, recalls McLaren's response to this sudden interest, and how she saw this merely as a re-articulation of the seminal influence of Waddington's epigenetics:

> I think in the hands particularly of Waddington … the attempt was to introduce epigenetics as a way of trying to integrate the two. And I think that that was a key influence in Anne's thinking and would have been an influence as well when she went to Russia. But what came much later of course was the rediscovery of epigenetics and the attempt at a three-way integration between development, genetics, and evolution. And this became known as evo-devo. And Anne was sort of cheering about it … I think we were both there, we were standing in line, to get into a conference or something, and she said, 'what is this evo-devo stuff?', we've been doing it all along! It's just got this fancy new name.
>
> Steven Rose, interview with author, 12 September 2018

The quote gives a sense of how McLaren delineated her object or, more accurately, her system of study as an epigenetic question in Waddington's sense – initially as a shared question for geneticists and embryologists alike. It also shows how she carried these ideas forward into an era that saw a temporary rejection of, and then a return to, these questions. Her science was, in a sense, always political, because she concerned herself with the

epigenetic questions that had been rejected in Western genetics and because these in turn belonged to a lineage of Marxist scientists all interested in challenging what they considered to be the individualistic models of Western science. Despite her Marxist politics, however, McLaren chose not to engage directly in political discussion or contestation of the issue, but, we will see, instead to the gradual filling-out of a continuous developmental model of gene–environment interactions that, over time, scientifically, built a body of research that was, undoubtedly, epigenetic.

McLaren, then, was good-humoured in her approach. We will see in detail how her career trajectory is testament to the trust she placed with scientific research to validate an open-ended, theoretical model. This allowed her to bring the disparate domains of developmental biology, reproductive biology, genetics, and even politics together through a characteristic, pragmatic, scientific approach. McLaren's research interest was in gene–environment interactions over generations, and built on Waddington's views – indeed, was consolidated when she joined Waddington's Institute of Animal Genetics in 1959 to explore the role of maternal influence in the process of implantation. This also led McLaren to develop her own model that resonates with Waddington's landscape in terms of not only its substance, but its translational ethos.

The scientist-explorer

McLaren's conceptualisation of her field of study is evidenced in her own descriptions of her scientific practice that I discovered in her archives at the British Library. I spent a considerable amount of time studying these laboratory notes, reflections and speeches, which are unusually detailed for a scientist's papers and thus provide valuable, textured descriptions of the scientific process. In the write-up of a talk given in March 1968 as part of an 'Evening Discourse' hosted by the Royal Institution in London, called *Life before Birth: the first few days*, for example, McLaren outlines the techniques used in her lab in order to explore gene–environment interactions in the first few days of development. After detailing the use of petri-dishes to observe mouse embryos *in vitro*, and chimeras to explore the mechanisms of differentiation, McLaren goes on to describe the methods used for studying implantation. She explains,

> If you spend, as I have spent recently, many hours looking at these successive slices of a pregnant mouse uterus, you acquire a most passionate sense of involvement with the mouse embryo and its problems. *The slices can be stained with different dyes which show up different features of the landscape – and it really does seem*

> *like a landscape, because your mind links the successive sections, like successive frames of a film, and you have the sensation of actually travelling through this long tube*. As you move along you note, with rising excitement the swelling of the wall of the uterus which heralds the approach of an embryo. One arm of the uterus seldom contains more than 10 embryos, each occupying only 10 or 20 slices, out of a total of several thousand slices, so one looks forward to the embryos like oases in the Sahara, or stations on the Trans-Siberian railway.
>
> McLaren, *Life before Birth*, 1968, pp. 14–15 [emphasis added]

Here, McLaren presents an analogy of the relations between the genes and environment, here at the level of the embryo as it implants in the uterus. The experiment is set up to bring to light the process of implantation by following the embryo in this process, describing the relationship between the embryo and uterine environment. McLaren is so immersed in her model that she describes herself as traveling through the uterus, awaiting the appearance of the embryo. McLaren, moreover, in describing her research object in terms that are very reminiscent of Waddington, as a landscape, also explains how this is constructed from a succession of images – a series of superimposed slides. It is through the relationship between these slides that a picture of the process of implantation begins to emerge. In order to move forward in time, the scientist performs a process of calibration to 'link the successive sections, like successive frames of a film'. The view that emerges is thus the product of a subjective relationship to an environment, and this relationship is enacted through a process of conceptually relating: each frame represents a set of relationships to the viewer and between elements in the environment. The question is how you get from one frame to the next to construct a temporal image of a process. The quotation shows evocatively how McLaren sees the work of science as the linking of layers, connecting these in order to build an image of a process, a model that, as Wadding vis-à-vis Serres, emphasises the subjectivity of the scientist as they move through the landscape and foregrounds the specific environmental context, the limited, partial picture that any single frame expresses. Any single frame, as an expression of a set of relations, contains all of the other frames, but each frame also only gains meaning in relation to every other frame. It is through that same movement between local observation and recurring jabs into the unknown that the scientist builds a picture. This process of relating by sequencing is performed from the limited perspective of the embryologist, and in response to a specific set of questions. We see how McLaren is thinking in terms of translations between contextual utterances, exercising her characteristic approach to link them in order to build a bigger picture that is both enabling and provisional, limited and receding, like the horizon

of a landscape.

I will now turn to a description of McLaren's career, which reflects this systemic way of thinking based on a conceptual and linguistic linking of layers by analogy. We will also see that, like Waddington, her model for doing science reflects a broader social vision enacted through a commitment to communication in and of science.

Pattern formation in developmental biologists

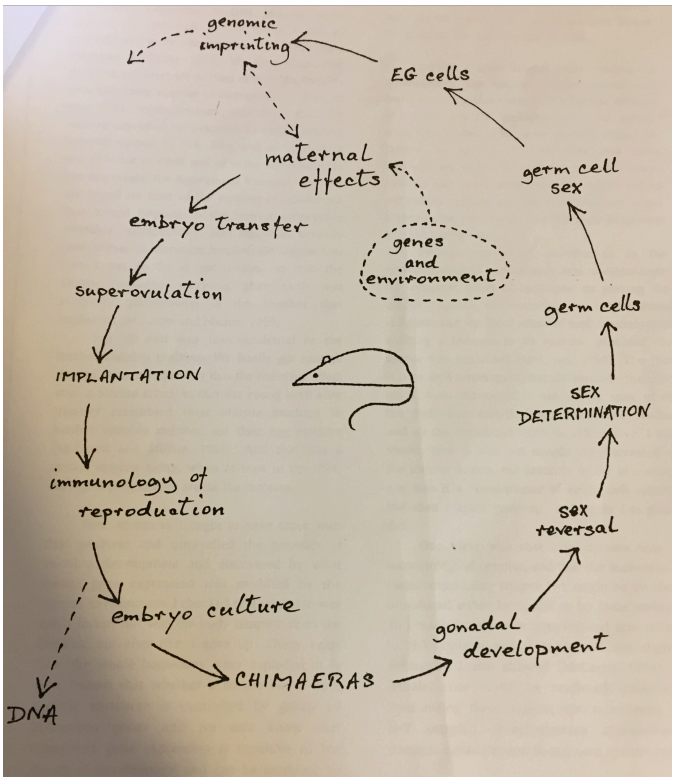


Figure 8. Diagram from "Pattern Formation in Developmental Biologists", 1997, Taniguchi Symposium, p. 58. In this figure, taken from a paper discovered in Anne McLaren's uncatalogued papers at her daughter, Susan Michie's house, McLaren summarises her own career as a pragmatic shifting between problems, thereby conceptualising her object of study as the entire course of the mammalian life cycle, from one generation to the next. Reproduced with kind permission of Susan Michie.

In an article presented at the Taniguchi Symposium in Japan in 1997, titled *Pattern Formation in the Developmental Biologist*, McLaren (1997) summarises her career by drawing an analogy to the development of the mammalian embryo. "In the developing embryo", she explains, "pattern formation occurs as the result of interactions between the cells' own genes and the surrounding tissue environment" (p. 57). She goes on, "similar interactions could play a part in fashioning the pattern of a biologist's career" (ibid.). She goes on to explore what she describes as her own "opportunistic" approach to biological research, which she contrasts to the approach of biologists who either pursue a single

research question throughout their careers, or those who invent new techniques. Those of the opportunistic school instead choose to focus on a "system" (p. 57). This means that they increasingly invest "intellectual capital" in this system and that, should it cease to "illuminate one particular problem, they shift to a different problem" (p. 57). McLaren's research career as she describes it is thus defined by "unashamed" opportunism and a particular pattern that reflects her interaction with her scientific environment (ibid.). This pattern, she goes on to show, is both "curiously cyclical" and characterised by a consistent interest in the interaction between gene and environment at different levels of organisation. She explains how she began her career in genetics, with an interest in how genes worked and how they interacted with their environment, which she explored in collaboration with her partner, Donald Michie, through their research in maternal effects on the number of lumbar vertebrae in mice at University College London (1952–1959). They showed that these were determined by environmental (uterine) factors and were not X-chromosome linked. This work led them to further develop the embryo transfer techniques devised in the Jackson Laboratory in the USA, including the techniques of superovulation, which later became instrumental to possibility of IVF.

Rather than fully exploring the effects of the uterine environment on gene expression, the embryo transfer experiments showed McLaren that there was a narrow window of opportunity for implantation, which led her to look into the conditions required for implantation. This, in turn, led to a foray into immunology, drawing on the view at the time that implantation might be an immunological reaction and that the male-specific histocompatibility antigen H-Y might be involved, introduced by sperm or male embryos. McLaren disproved this view and also showed that female mice could be rendered infertile by immunising them against sperm antigens (not H-Y antigens), foreshadowing approaches to immuno-contraception in humans. At this time, McLaren began not only transferring, but also culturing embryos *in vitro*. She showed, with John Biggers, that mouse embryos could be cultured for several days using the technique developed by Whitten (1956), and could then develop into normal, live fertile mice after transfer (McLaren and Biggers, 1958), a discovery that had obvious relevance to the development of IVF in the human.

The work on embryo culture then led to investigations into aggregation chimeras, using the techniques described by Tarkowski (1961) and Mintz (1962). This work was of interest from the point of view of gene–environment interaction because it meant that for the first time in mice, cells of one genotype could differentiate in the cellular genotype of

another. This work led to a focus on the development of gonads and germ cells in sex-reversed mice, and then a return to the question of the male-specific histocompatibility antigen H-Y, which was believed to be the male-determining factor. McLaren worked with Elizabeth Simpson (1984) to refute this hypothesis, showing that H-Y was not necessary for male development. She continued to pursue this interest in germ cells and looked at how they develop into gametes, as well as the origin of the primordial germ cells.

Finally, McLaren became interested in immortalised embryonic germ (EG) cells that are derived from germ cells, which resemble embryonic stem cells, in that they can proliferate indefinitely in culture and can contribute to all cell lineages if introduced into a blastocyst. This turn to EG cells finally brought her back to the interest in parental effects and gene expression that had started her career. These parental effects by this time were known to be mediated by genomic imprinting. EG cells could be used to investigate when, in the germ cell lineage, the parental imprint is removed, and the new imprint is imposed. This completed the cycle of McLaren's interests, and the cycle of the mammalian life.

McLaren accordingly saw the path of her career as an expression of the correct object of study for the embryologist, that is, "the entire life-cycle of the mammal, from one generation round to the next" (p. 61). This holistic approach to development necessitates drawing on the approaches and foci of "not just developmental biology, but also reproductive biology and of course genetics" (p. 61). "To me", McLaren writes, "that is all one subject, but it has no name" (p. 61). McLaren, then, studied the interactions between gene and environment, the expression of genes, and the reproduction of a species. McLaren's scientific approach encompasses all of these perspectives into a cyclical study of mammalian development from generation to generation. Germ cells are key to this model, they form what you might call the 'baseline', as they are the cells that carry developmental information between generations, rather than the gametes that fuse to form the finite individual. Rather than the development of the individual, it is the interaction between gene and environment that is central to McLaren's research orbit.

Waddington's echo is very much heard in McLaren's model of her career. In conceptualising her object of study as the entire course of the mammalian life-cycle, McLaren does not commit herself to a single disciplinary approach; what interests her is the entire system, which she explores through a series of analogies that biologically model the same set of relationships of gene and environment. This modelling of her system in terms of a set of analogies operating at different levels of organisation means that there is

no privileged level of analysis, and it is only by their metonymic relation to the abstracted theoretical model of gene–environment that each of these layers is interesting. It is only once McLaren has performed the work of integrating the models into her broad vision that they become meaningful.

McLaren's overview of her career also suggests how she was as committed to producing what was the aim of Waddington's epigenetics – a causal analysis of development. This required transgression. Serres describes in reference to his third science, he "multiplies" and "disturbs" the "boundaries" between fields, "preferring disorder and fertility to sterile order" (Squier, p. 13). Waddington similarly writes in *Strategy of the Gene*s (1957) about the sense in which embryologists cannot rely on a single level of analysis, or linear account, explaining the need for a multidimensional visualisation. The study of development should concern itself not just with "the final state to which the system arrives, but also in the course by which it gets there" and the "best image of the epigenetic landscape ... would have been in a "phase space", or "a system containing many components [that] can be represented by a point in multidimensional space" (p. 125). The embryo in development becomes part of a layered account that incorporated several scales: "the time of embryonic development, the time of an organism's life span, and evolutionary time" (Squier, 2017, p. 85). Such an approach is concerned "less with individuals than with the relationships and processes that continuously constitute being, in its multiple changing forms, in time" (ibid.). The embryo is linked to a population (of embryos) that precede and follow it:

> The epigenetic landscape depicted both the specificity of embryological development (at the scale of the fertilised embryo) and what he called "the full biological picture" made visible by the mediation of the microscope as well as statistics. The ongoing metabolic and physiological processes of development over the lifespan connect the embryo both to the "broken-down nag it will eventually become" and to the population from which they both have emerged and that they constitute in turn.
>
> Squier, 2017, p. 84

McLaren's research concerns itself with this same broad biological picture that links the development of the embryo to inheritance. McLaren's scientific approach, then, relies as much on the superimposition in her science as she does in her policy work. Here, too, layers of organisation are superimposed in order to build a working model of a problem that always exceeds what can be known. In both cases, McLaren draws on her abilities as

a connector, a clarifier to build to a broader vision and, in doing so, reveals the interconnectedness and the complexity, the infinite regress that defines a scientist's 'understanding' of anything. It is only through this process of translating between analogies, of transplanting her model into different contexts and allowing them to be changed by their specific environment, that McLaren is able to develop her biological picture.

Scientific analogies as translation

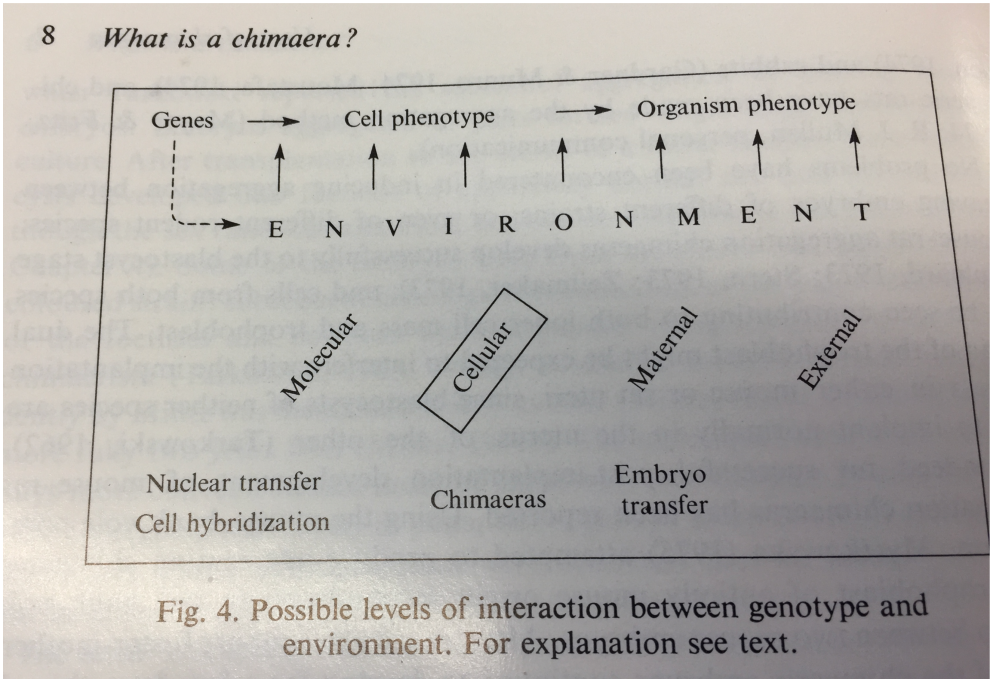


Figure 9. Diagram from *Mammalian Chimaeras*, 1976, p. 8. Here, McLaren schematises her research interests as analogies of the same nature-nurture problem, which she sees as the relationship between the gene and its environment.

McLaren's description of her own career also makes explicit the extent to which her view emerged from, and was expressed through, the drawing of analogies. In the same Taniguchi speech, McLaren goes on to describe how she advanced her scientific career through her ability to draw analogies between fields, specifically between levels of organisation. Take for example her interest in tracing cell lineages through the development of chimaeras. In her *Mammalian Chimaeras* (1976), she explains how this is really a more sophisticated articulation of the nature-nurture problem that had dominated biology,

> The old nature-nurture problem ... was concerned with how an organ of a particular genetic make-up interacts with its environment to achieve its final adult phenotype, and how much of this phenotype is determined by the genotype and how much by the environment. In mammals, the environment includes not only the external

> conditions to which the individual is subjected after birth, but also the maternal environment that it experiences before birth. The technique of embryo transfer can be used to investigate the importance of maternal environment ...When we consider the development of individual cells, we are faced with a more sophisticated extension of the nature-nurture problem, since the environment in which a cell develops is made up of the population of other cells surrounding it.
>
> McLaren, 1976, p. 6

This passage, like the outline of her career trajectory, shows how McLaren links different systems metonymically through an interest in the individual–environment relationship. Like Waddington, she constructs her scientific object as a broad, abstract set of hypothetical relations, which she then explored by modelling a node in this system at various levels of organisation, intervening in biology in order to orientate herself in a landscape and to describe what she sees. The biological model, in this way, does not represent an object to be fully explained, but a set of relations that are never fully understood, most likely inexhaustible for a scientist in their lifetime. As Waddington describes in *Behind Appearances* (1969), a piece of existent stuff contains everything you can find in it – you'll never exhaust what it contains: "you may find a fossil, and that is an item in which the whole pageant of organic life on earth is focused" (p. 113). McLaren had her own fossil in the form of the spiral, as we saw in figure 1, which expresses the unfolding of development as a series of events that comprise the interaction between layers of organisation over generations, a process that is cumulative, interactive and generative. This spiral is in turn expressed in a series of biological analogies, each of which represents a particular biological layer, as we have seen here, which is in turn interpreted and calibrated in reference to the infinite spiral – McLaren's own version of the epigenetic landscape.

Here, then, we are given a clue as to how to characterise McLaren's thinking in terms of superimpositions as a broader ethos – experiments express individual metonymic expressions of a broader biological vision; her models are forays into an infinite landscape that takes us from 'one generation to the next'. The same sense-to-sense, Derridean translation, the same 'merographic thinking' in Strathern's (1992b) terms, used in her policy model, is very much at work in McLaren's approach to science, as she moves between metaphoric reinterpretations of the central problem. At the time that McLaren was doing her science, however, this subject had no name. 'Epigenetics', the term that Waddington had introduced, had fallen out of favour following the rise of molecular biology in the 1960s and the accompanying centrality of genes to models of development.

Here, as in the debates on human embryology, McLaren was forced to invent models and languages to express the work she was doing across that would connect resources across domains. The speech she gave at the Taniguchi Symposium in 1997 was one such attempt at a broader conceptualisation.

Making science heard

This model for doing science, this way of seeing the research object, resonates not only with Waddington's translational model of the landscape but also with the similarly relational epistemological model described by feminist critics of science. Evelyn Fox-Keller, in her biography of the geneticist Barbara McClintock (1983), for example, shows how McClintock's particular style of practice as part of an attempt to explain why her theory of gene transposition failed to be 'heard' by the scientific community in the age of molecular biology. Here I return to McClintock's style of practice introduced in the methodology section of this thesis vis-à-vis Keller, and compare this to the descriptions of McLaren's science I have given, in order to foreground the importance of communication to McLaren's practice.

Science as persuasion

McClintock's method, as we saw in the introduction, revolved around acquiring a holistic understanding of the organism as a whole, a 'feeling for the organism', that then allowed her to make sense of her observations. McClintock's description of the internal process of integrating her data to become part of the system she was studying resonates with the topographical language of landscapes McLaren uses to convey a similar sense of a process of theorising and integrating to make sense of her laboratory data. McClintock's former colleagues in this way recall how it "was easy for McClintock herself to lose sight of the difference between what could be seen by the relatively uneducated eye and what could be seen only with the help of a long chain of logical inference that, to her, had become second nature" (Keller, 1983, p.126). Her investigations into transposition required "a prodigious amount of cognitive processing" that "intervened between the spots of pigment she could actually see on the corn plant and the controlling elements she ultimately came to write about" (ibid.).

McClintock uses the analogy of a computer to describe the cognitive processing of the data she saw. This computer was "mediating between the spots, the patterns they

formed, and her internal vision" (ibid). It was this internal vision that provided a constant "point of reference" – an "organising scheme" (ibid). It was a process of moving between the model in her head, which expressed a holistic understanding of the organism, the data in each frame, and then in succession. It was a temporal reconstruction of a process in constant reference to an internal model. Like McLaren looking at the slides, the modelling process performed by her internal 'computer' mapped the relationships between slides over time. Each slide only becomes meaningful in relation to the others through calibration by reference to an internal model.

Yet the comparison between McClintock and McLaren also reveals a distinctive difference. It is evident, on the one hand, that the two scientists shared a commitment to an understanding in terms of the entire organism. McClintock was opposed to the reductionist tendency in genetics and her commitment to understanding the whole organism meant that she maintained an interest in embryology besides the precise mechanisms of cytogenetic processes. Her method, then, like McLaren's, centres around understanding the relations between systems and this requires calibration vis-à-vis a model that she describes as 'intuitive', that is internal, but that comes to resemble the 'neutral epistemic object' of Waddington's landscape, or McLaren's conceptual spiral. Yet despite the similarities between the ways in which McClintock and McLaren describe their way of integrating and calibrating their biological data, for McLaren, this model has implications for her role as a scientist beyond science and provides a framework for collaboration and communication across fields and even entire domains of knowledge.

This differs greatly from McClintock's isolationist approach. Keller describes how McClintock's way of working through experimental questions emerged "in her own highly individualistic way, dictated more by internal than external forces" (p. 268), and shows that McClintock made little effort to make her science more broadly understandable, even to colleagues. Her deeply isolationist mode of working, Keller argues, explains in part the delay in response to McClintock's theories, which was not only a result of their "revolutionary implications" in the historical context of genetics, nor even just her "particular nature of her knowledge and understanding" (p. 268). Indeed, the brief comparison to McLaren's description shows how comparable their views are. But this was in part attributable to the fact that McClintock had no interest in communicating her findings in the conventional language of science.

This task of what Keller calls 'persuasion' shapes the interpretation of evidence,

relates it to existing knowledge in a particular way, and determines the success or failure of a discovery. McClintock did not always play by these rules – in fact, in Keller's account she seems to actively cultivate a myth of her science as impenetrable, her methods almost too distinctive to describe. She is adamant that her approach is unique, that her mind was "processing and integrating data far more complex than we can possibly be conscious of" (Keller, 1983, p. 103). Her understanding, she says, relied on a "computer … working very rapidly and very perfectly" and, she says, she "couldn't train anyone to do that" (ibid). McClintock, then, did not seem to be amenable to the persuasive work required to make new ideas in science 'heard', to appeal to the common assumptions of what is logical and rational, refusing to adhere to a convention in which certain discourses are privileged over others.

McLaren presents a very different case. As we have seen, her biological vision is expressed in a career path in which she moved between disciplines as a result of her ability to collaborate by translating and mobilising people across fields to work on various iterations of her core interest. Connecting people and resources through the cultivation of a shared language was thus integral to her practice. McLaren applied her biological methodology of integrating information across fields, to a much broader conceptualisation of scientific work that included forging social bonds over shared understandings of a scientific problem and, thus, could not have been further from McClintock in her commitment to clear communication. McLaren was, however, as we have seen, marginal to the scientific community in a different sense, largely and paradoxically as a result of her commitment to this 'social' aspect of science, in that she was actively working in science policy as well.

As we have seen, her increasing involvement in policy discussions from the Warnock Inquiry onwards led several of her former colleagues whom I interviewed to assert that, from the 1980s, McLaren was "scientifically past her prime" (Goodfellow, interview with author, 21 September 2018). Interviewees also repeatedly drew a comparison between McLaren and Mary Lyon. The latter was a real 'Nobel' scientist, whereas McLaren was a great 'absorber' and a 'connector' (Gardner, interview with author, 27 September 2018). A distinction is repeatedly drawn by those interpreting McLaren's legacy today between the social work of collaborating and doing policy work, and the work of basic science. Again, as we saw in the previous chapters, the scientists I interviewed distanced themselves from the 'non-scientific' realm of policy-making and public debate, by deeming terminology

such as the ‘pre-embryo’ arbitrary or, in this case, by casting McLaren’s role itself as unscientific.

It is unthinkable, it seems, that the social work of absorbing and connecting might aid scientific practice, or that scientific thinking might instruct the scientist in their contributions to policy; yet McLaren, we have seen, was committed to both these activities. In the case of her science, I have described how her approach was ‘social’ in several regards: in the sense that it relied on a relational epistemological model for her relationship to her research object, and also in the sense that she mobilised a range of resources and research trajectories through collaboration and communication across three fields. In her policy work, similarly, we have seen how McLaren also applied this relational biological thinking, and equally communicated and absorbed logics and views that fell outside of the boundaries of science into her biological case. In both contexts, the distinction between ‘social’ and ‘scientific’ work is much less clear-cut than the statements by scientists regarding her role imply. For McLaren, at least, these represented two necessary aspects of solving a problem. The processing required to integrate evidence into her internal model, for McLaren, also meant doing the social work of communicating, connecting and translating, because society is very much a part of this machine.

Indeed, a minority of former scientific colleagues, such as the biologist Elizabeth Simpson, credit McLaren’s ability to connect in order to facilitate the development of ideas as the strength of her science. In her 2001 article, *The Case of the Midwife Scientist*, she reveals by implication, how the disregard for her style of practice is also gendered. Simpson writes,

> She has managed to juggle manifold demands of students, colleagues, friends and family, as well as wider demands in the scientific community. The fact that she is a woman does not make that a more remarkable feat – as she once said to me, women used to coping with small children have the knack of doing several things simultaneously.
>
> Simpson, 2001, p. 517

The work of connecting and collaborating, associated with the ‘female’ practice of socialising, in order to move a broad agenda forward, is considered by many members of the scientific community as secondary to, and less remarkable than, the serious, and by opposition, ‘male’ activity of the conception of big ideas. McLaren’s scientific approach is based on a very different premise, in that evidence is constituted in a very different way, through the interpretation of analogies that are developed through collaboration across

fields, rather than the construction of singular breakthrough experiments. Simpson, in an interview with me, revealed how this approach was nonetheless deeply scientific:

> … She thought about whole animals and what made them and she couldn't separate out –she mercifully didn't separate out – her thinking about the cells and the DNA from the whole animals and the phenotype. She was somebody who had the ability to think through the whole process and devise experiments for getting at things and if she thought it would be good to look at things a certain way and have the means for doing it, she'd ask someone to help her to do it.
>
> Simpson, interview with author, 21 June 2018

McLaren constituted her scientific evidence through collaboration. The work of getting ideas taken up, I have shown, is highly conceptual, and requires an intimate understanding of the scientific problem, as well as the capacity to model this across domains, in addition to the capacity to communicate and collaborate. Midwifery was integral to McLaren's science.

Technological pragmatism

There is also another component of the scientific style of practice that McLaren shared with McClintock, but that McLaren once again also enacts in the form of a social, collaborative approach to communicating science across fields; namely, an embryological, tool-driven experimental ethos. Franklin (2013a) has drawn attention to the role that tools have played in embryology. She writes a history of IVF in which she shows how this technology came about as the result of a legacy of embryo pioneers 'taking biology in hand', a lineage of techniques that always extended beyond what was 'known' about the biological system and that increasingly blurred the line between biology and technology, collapsing them into a biological model where the "layering of tool, organism and experimental system to model the fusion of internal organising forces and externally imposed mechanical technique – what it represents becomes confused – technology and the concept totally entwined" (p. 125). In each case, the technique facilitated another, putting biology to work to build a biological picture over time. This culminates in the modern-day ethos of synthetic biology, where biology is taken in hand in order to make it 'work'. And just as biology thereby becomes relativized, as we have seen, technology too is biologised in the form of new living tools:

> From the point of view of the evolution of technique, it is irrelevant that much of this work was experimentally inconclusive, misleading, or failed – because much of it was not result but technique driven to begin with. Its larger object was not only

> modelling biological mechanisms, or for that matter reworking them, but building a new biology in which tool and substance work together biologically.
>
> Franklin, 2013a, p. 135

It is exactly this circulation of tools, this technological probing, that McLaren's career exemplifies; an engineering ethos that makes biology 'work' and substantiates a biological model over time, filling out a hypothetical picture through the movement between models and techniques. As we have seen, McLaren extends this approach to her policy approach, encouraging the development of clinical applications such as PGD, building research agendas accordingly, but also referring to clinical translation as a projected horizon representing shared values. The movement towards this moral horizon is executed through the development of techniques, which are entwined with biology, so that the biological model comes to embody this shared, projected, moral vision and showing how "experimental embryology", in the context of policy negotiations, too, represents "an accumulation of techniques that evolve through circulation, as they are passaged through a range of contexts, becoming interwoven with a diverse set of fundamental and practical problems in the process" (Franklin, 2013a, p. 111).

McLaren's career and role in the Warnock debates shows how this technological kinship is established through communication and relies on shared epistemic objects; how her techno-biological models become analogies for a broader system, and how her great asset as a scientist was therefore not only 'absorbing', but once again, constructing a biological model by translating across domains to build an area of overdetermined convergence. Using the technology at her disposal across various fields, she drew on physical analogies across fields that brought research together over a shared problem, connecting this research to her overarching model of gene–environment interactions. This description of her career also reveals once again, how McLaren provides a case in point of how science involves a broader range of activities and ways of thinking than scientists themselves are prone to acknowledge. The distinctions between facts and values, social and scientific, feeling and rationale blur when science is framed as a process of making model systems 'work' in order to inform – by analogy – a growing understanding of a theoretical system. This pragmatism, in turn, as we have seen, also guided McLaren's thinking in the policy realm – making clinical applications work in order to recruit a wider constituency to her biological model, providing, as it were, 'proof of concept', but also shaping people's relationships to biology through an iterative process of incorporating ethical, emotional and

legal perspectives into the biological account. Indeed, the 'social' aspects of her work and the mode in which McLaren approached science also reflects a specific conceptualisation of the relations between science and society more broadly. For McLaren, we will see, methods and approaches travel between science and society because these domains are intimately entwined. Doing laboratory work, even in the collaborative and more broadly encompassing sense described, to McLaren, was not enough. Indeed, she claims,

> A philosopher once said: "The aim of philosophy is not just to understand the world, but also to change it." The same is true of biomedical science: it's not good enough just make do with palliative measures for all the most intractable medical conditions like arthritis and Parkinson's and multiple sclerosis and diabetes – we must try to cure them. The use of stem cells would seem to offer the brightest hope at the present time. Whether it's adult stem cells, foetal stem cells, donated embryonic stem cells or nuclear transfer cloned stem cells – which will work best, only research will tell. To me, this is a much more important potential consequence of the experiments of Loeb and Spemann, Briggs and King, Gurdon or Ian Wilmut, than the possibility that somewhere in the world, one or two cloned babies may be born.
>
> McLaren, *Thanks-MSU*, British Library, Add MS 89202/2/13, undated.

Science, for McLaren, always meant looking to what science could do for the health of society – not just in the field of reproduction, but broadly. Her participation in the human fertilisation and embryology debates is a very clear manifestation of this conviction and, as we have seen, was expressed through the language of clinical translation which reflected the shared values that connected science and society. While this commitment to 'extra-scientific' activities as part 'doing science' are not unique to McLaren and, indeed, other scientists have spoken about their commitment to this way of thinking, it was this awareness of the function of these activities, her ability to describe them, and a sense of social responsibility expressed in an active public role as a result, that makes McLaren's style of practice so distinctive. I will now draw on McLaren's own description of her conception of the science-society relationship to show how her approach to both science and policy-making are part of a problem-solving ethos that relies centrally on translation – of connecting domains through the ongoing cultivation of a shared language.

Scientific social responsibility: A problem-solving ethos

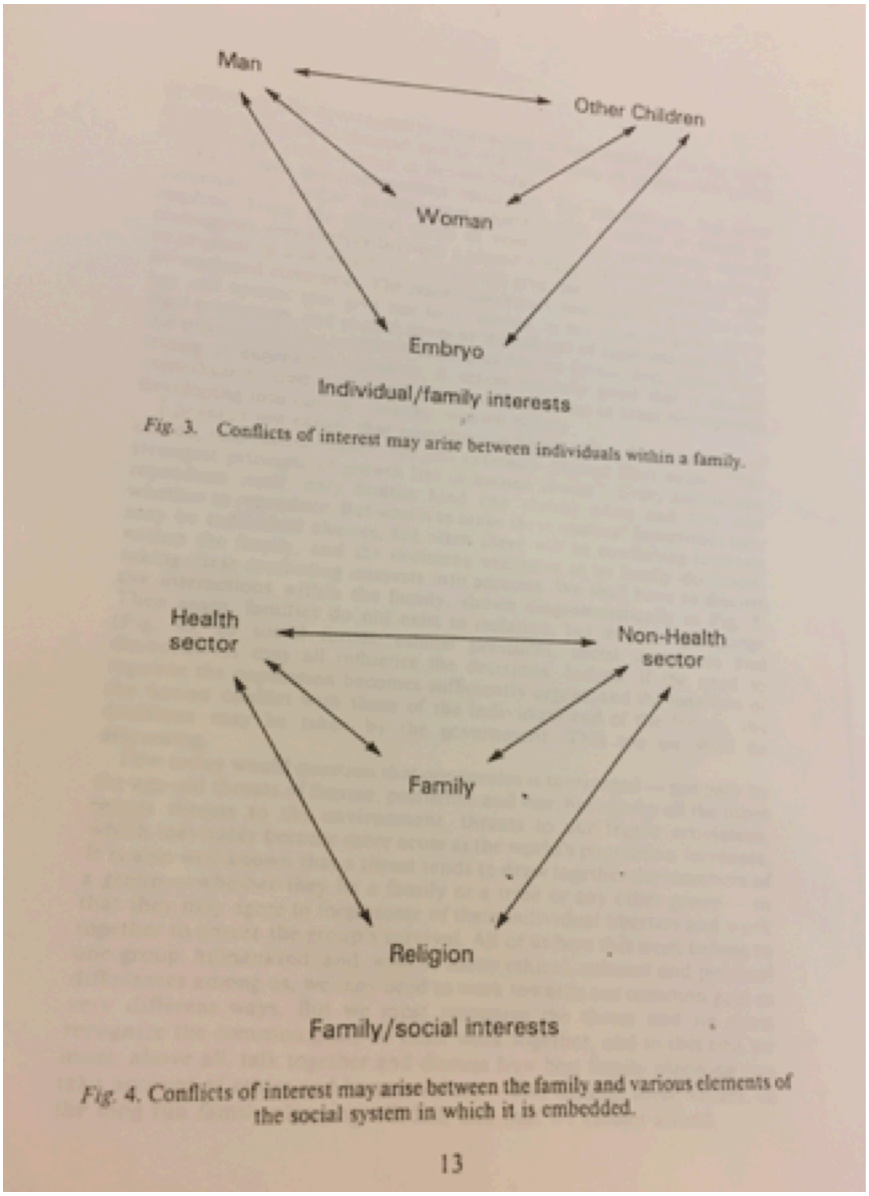


Figure 10. Diagram from World Health Organisation speech, 1989, British Library, Add MS 83980. In this diagram, McLaren schematizes the conflicts that may arise as the opinions and interests of various levels of social organization are incorporated into the process of decision-making about family planning. Because these conflicts are unavoidable, she argues, it is necessary that they are represented in public debate as the only socially responsible basis of inclusive, representative policy on reproductive technologies. All Anne McLaren Papers images reproduced with kind permission of Susan Michie.

Besides her work in the HF&E debates and the numerous other professional and political positions she held, McLaren was also committed to the cause of family planning, particularly in developing countries. Given that these meetings often addressed a broad range of issues related to reproduction, demanding explicit attention to both social, systemic issues of provision, access, and the reproduction of society as well as the reproductive technologies themselves, these meetings provided a context in which

McLaren was able to articulate her vision of the science-society relationship more broadly. In one keynote address given at the World Health Organisation in Geneva in 1989, of which I found a handwritten copy in the McLaren papers in the archives at the British Library (McLaren, 1989, *Geneva WHO Keynote*, British Library, Add MS 83980), McLaren describes her social model for the role of reproduction in society. The speech reveals how McLaren saw the communication of science as integral to the work of the scientist, and as the only means of ensuring that science fulfils its potential to better society. She begins,

> As a biologist, I have worked all my life in reproductive and developmental biology, always on animals, but always with human problems in mind. I have worked on, for example, induced ovulation, the regulation of fertility by immunisation against sperm, and *in vitro* fertilisation (IVF) and embryo transfer. When it became clear in the early 1970s that it was only a question of time before IVF and embryo transfer became a clinical reality in our own species, I became concerned with the ethical and social and legal implications that would ensue. *It seemed important that the benefits that this new technology could bring to childless couples should not be threatened by public ignorance and fear of the unknown.*
>
> McLaren, *Geneva WHO Keynote*, 1989, p. 7, Add MS 83980 [emphasis added]

McLaren details her personal awakening to the so-called 'ethical' implications of her basic scientific work, which emerged, as she says, in response to a growing awareness of the relevance of her work to human health, thereby echoing Warnock's insight that basic and applied research became increasingly entwined for McLaren over the course of the debates in a model that represented the good that science could do for society (Warnock, with author, 21 August 2018). As we have seen in the previous chapters, clinical applications thereby came to represent a particular relationship between basic science and society, in which science served the public good. Clinical translation gained its meaning, then, through its relationship to a set of other domains, all of which came to converge through a series of translations. In this particular talk McLaren emphasises the broader, social context of clinical applications, showing that social benefit is not intrinsic to technological advance, and emphasising the 'extra-scientific' work that needs to be done to secure the benefits of clinical translation. What particularly concerns her here is that people are adequately informed. McLaren here spells out how finding a common language is key to a fair science that serves the social good.

In her speech, McLaren outlines her involvement with the WHO Special Programme of Research. The programme set up in 1972, that went under the full title of the WHO "Special Programme of Research, Development and Research Training in Human

Reproduction", upheld, as the name suggests, a trinity of responsibilities (*Geneva WHO Keynote*, p. 7). First, it concerned itself with "funding and coordinating research on human reproduction on a global scale", secondly with "improving the potential for such research in developing countries" and, finally, it recognized, "from the start…the importance of ethics in family planning and research", having played a significant role in establishing ethical guidelines in the field, not only ensuring that its own programmes adhere, but also encouraging the setting up of local ethics committees (pp. 7-8).

McLaren casts the development of the programme and its aims against a backdrop of scientists' gradual awakening in the 1960s to the world population problem and, at the same time, to the high levels of maternal mortality. She details how over the course of the Special Programme, there has been a movement from mainly basic research to developing applications, combined with research into the "social factors that affect birth rate", such as "the acceptability of different contraceptive methods, as well as health-services research in family planning and studies on the social and behavioural determinants of fertility regulation" (p. 8). This vision of the relationship between research and application is still rather conventional; closer to the 'bench to bedside' model that we know today (Cambrosio et al., 2006), in which basic scientific research is channelled directly into clinical applications that are then disseminated to 'society', a distinct domain. Research into so-called 'social factors' appears as an afterthought, providing suggestions for small adjustments to the implementation of clinical techniques after the fact of their development; society does not appear to be seen as co-constitutive with the development of the clinical techniques.

Importantly, however, McLaren goes on to show how the Programme also began to expand its remit beyond this early emphasis on contraception – building on the insight McLaren described as emerging in the 1960s, that the problems of maternal mortality and population growth might both need to be addressed under the same paradigm. Over the years, she explains, the "Programme has broadened its whole interpretation of family planning to include projects concerned with, for example, the causes and alleviation of infertility, in addition to its original emphasis on development of contraceptives", which leads McLaren to state her own view on family planning, or "family decision-making as it is sometimes termed", claiming it "is a very broad one" and "includes all those areas of reproduction where couples today – or maybe tomorrow – can exercise responsible choice, rather than leaving the outcome to chance" (p. 9). The examples she gives include:

“Whether to have children? How many? When? Which sex? If infertile – whether to seek treatment? Assisted reproduction? … Whether to request prenatal diagnosis for genetic disease? Whether to terminate an affected pregnancy?” (p. 9).

As in her science, McLaren takes a broad view of family-planning that is guided by a central, pragmatic, question much like the question posed by the Warnock Inquiry and, as we have seen, the one that provided the impetus behind her movement between scientific analogies. In this case, too, McLaren asks, ‘What can we do using the tools that we have?’ This new question, relying on a broader conception of family planning, which actually reflects a view of the role of reproduction in society, leads to a different mandate for the WHO Special Programme and, indeed, for the individual scientist. McLaren stresses that, while “a good deal of basic mission-oriented reproductive research still goes on, and this is also important because it strengthens our understanding of human reproduction, and the knowledge gained can be applied to either the downward or the upward regulation of fertility” (p. 8), it is now equally important that scientists not only provide options, but also the information required to make decisions when choosing between them.

> The new options that are available allow couples more choice, but the other side of that coin is that they require couples to take more decisions – and if they are to take responsible decisions they need information.
>
> McLaren, WHO, 1989, p. 8

Here we hear echoes of the comment made by Bolton in the previous chapter, that when faced with the vote on human embryo research, it was important that people were well-informed, or they would be likely to vote out of “gut reaction and motivated my prejudice and misinformation” (Bolton, interview with author, 16 October 2018). Bolton goes on to draw an analogy to Brexit to emphasise what is at stake in the failure to inform, namely that a vote is no longer a deliberate choice for a defined outcome, but a stab in the dark, and so, not really a choice at all. Here McLaren similarly shows that the pragmatic aim of clinical translation only serves its social function when scientists and the public at large are able to negotiate these options in a shared language. Informing the public is not a matter of imparting a literal scientific account, but of negotiating choices. A negotiation relies on an iterative exchange of perspectives, values and information that will come to be expressed in a shared ‘language’. Providing options is in itself insufficient and, in fact, unethical if it is not supplemented with the language required to choose. Problem-solving for the scientist, in this instantiation, is more about empowering potential patients in their

decision-making than solving the problem for them. The scientist must take up this supportive role as the necessary complement to the technological assistance to reproduction. Clinical translation, then, as in the Warnock discussions, represents to McLaren a nexus between science and society, which points to both the potential good that science can do in its social context, as well as an imperative for the scientist to do the communicative work required to facilitate this good.

The work of superimposing clinical, scientific and social visions and calibrating them into a shared model, hereby becomes more than an idiosyncratic problem-solving approach, but a problem-solving ethos, a style of practice representing a pragmatic moral stance that is characteristic of McLaren's approach across all the contexts in which she worked. McLaren's broad vision that encompasses everything required to get 'from one generation to the next', when carried forward into policy work, thus fulfils the potential of Waddington's epigenetic landscape as articulated by Susan Squier once it is transported beyond its original context, to facilitating an expansion of what are classed as the 'environmental' factors in accounts of development, which can come to encompass a whole "ecology" of social factors beyond those in molecular or cellular biology (2017, p. 183). McLaren goes on to articulate how the role of the scientist changes under this new ethos of 'family planning' as supported decision-making:

> Scientists exist by courtesy of society – society pays them, and in return they have the responsibility of providing its members with information. In the present context they have the responsibility of telling people the facts about human reproduction. We know that every society has its ethical vision of human reproduction … but we also need a scientifically based vision of human reproduction that can be superimposed upon the ethical vision, in the same way that one can superimpose two images upon one another by converging one's eyes. *To superimpose these two visions is a necessary basis for responsible decision-making.*
>
> McLaren, WHO, 1989, p. 10 [emphasis added]

Here, McLaren summarises the problem-solving ethos that I have described over the course of the previous two chapters. This ethos that she, in her characteristically understated manner calls simply 'responsible decision-making', is enacted as the process of superimposing a scientific narrative onto a social vision so that both remain visible, never perfectly or definitively aligned, but also mutually altered on route to a shared clinical goal. McLaren then, presents this superimposition, which I have shown is translational, as an ethical imperative when science is positioned as a source of social potential. This is the true process by which the potential of clinical translation is harnessed. By superimposing

social and scientific visions in order to find a suitable language that encompasses the fractured but overlapping concerns and values of both domains, scientists fulfil an ethical responsibility to provide not only possibilities, but to empower potential users to make a choice in how society moves from one generation to the next.

Interestingly, McLaren goes on to propose a model for decision-making in family planning that resonates with the one I have described for the Warnock discussions. Pointing again to the possibility of choice in reproduction, she asks who is to make that choice. She schematises the various layers that need to be considered (see figure 10) – individual interests; then family interests; then social interests – and shows how these are often in conflict with one another. McLaren writes that it is essential in moving towards a solution that they "recognise their common goal", which she describes as "the group's survival" in light of the "threats of famine, pestilence and war … the environment. … our fragile ecosystem" (p. 14). This shared aim, a horizon of hope, provides the context for an imperfect, common ground. Like clinical translation, it is within this explicit framing of a common goal, that discussions can effectively take place:

> … we must work together, and to this end we must, above all, talk together and discuss how best family planning can take account of ethics and human values – for without, these values, in the long run family planning will fail, and that we cannot afford.
>
> WHO, 1989, p. 14

The ethical and human values are negotiated through the very process of articulating and arriving at a 'common goal'. Again, we seen replicated the Warnock process of superimposition, of incorporating various concerns in order to move towards a common ground representing shared values. Again, we see that, for McLaren, the clinical application does not in itself fulfil this criterion, but that it can come to represent, as it did in Warnock, a shared moral goal that facilitates convergence. As the scheme of the various interest groups in figure 10 shows, McLaren saw any solution as temporary, imperfect and built on fractured ground, but an acknowledgement of these irreconcilable tensions is a prerequisite for an inclusive negotiation of directions in science, for reproduction as informed decision making. The speech, written in 1989 and thus towards the end of the Warnock discussion, shows McLaren's growing sense of her moral position as a scientist, one in which she does not provide an answer but lays out a framework for thinking through the problem, bringing clarity and distinctions by layering various logics: a rational, scientific process that relies

on social collaboration, affective understanding, and iterative deliberation, all of which is required to harness the power of scientific exploration for the social good.

McLaren and socialist science

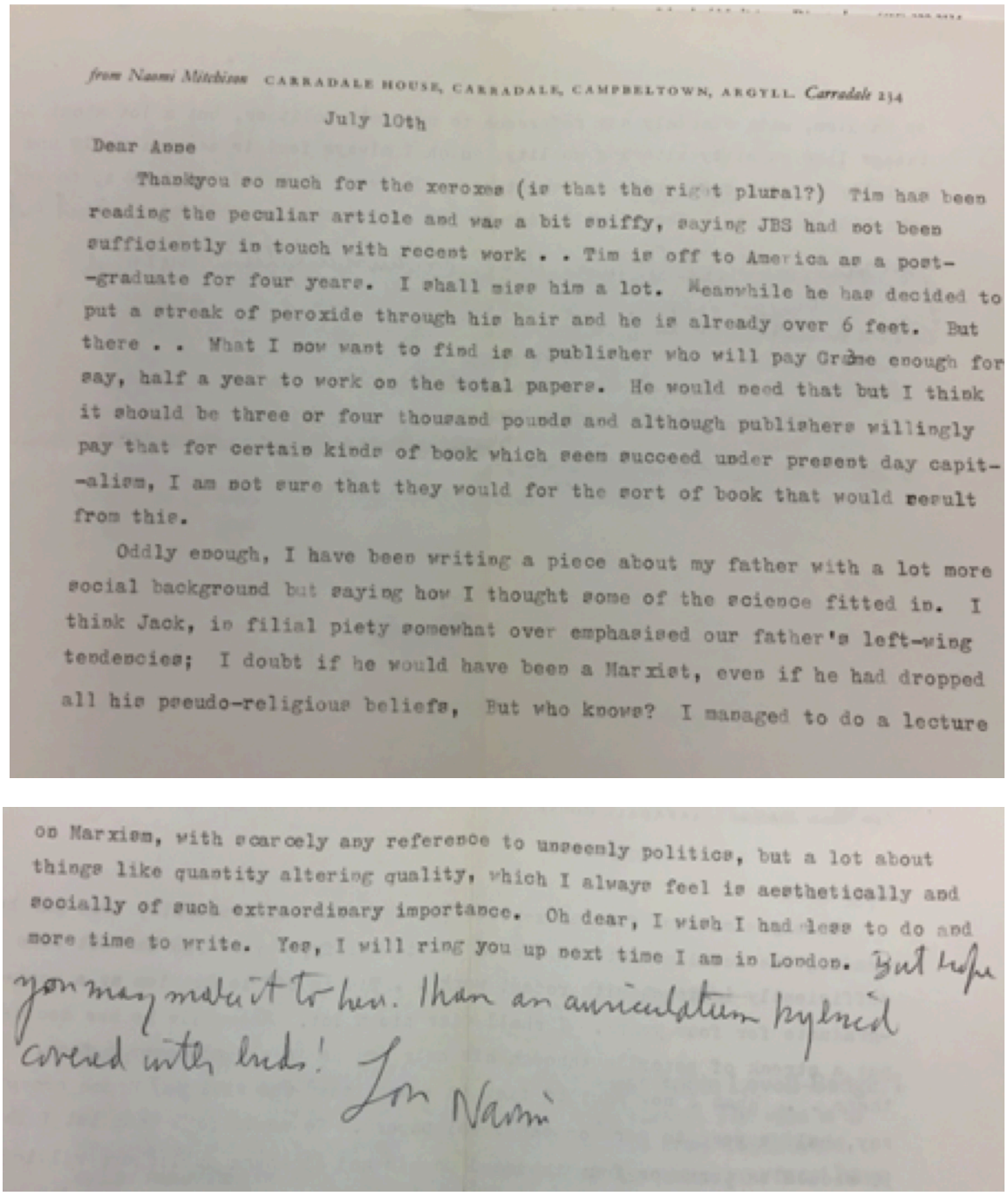
I have described McLaren's vision of the relationship between science and society, one that pivots around clinical translation and its ethical imperative to support public engagement around science. I have also shown the palpable influence that Waddington, a notable socialist and embryologist, had on the problems and the approach McLaren took. This style of thought, as we have seen, later led McLaren to a conviction that the scientist has a responsibility to communicate, to work towards a shared language by offering relevant scientific information to the public in order for science to deliver its positive social potential. Here, I turn to McLaren's education and politics more broadly, to further show how her views on the role of science in society were enacted through her imperative to move between disciplines and domains to expand her theoretical model.

Moving between genres

It is perhaps unsurprising that McLaren initially intended to pursue English at Oxford, given that she was surrounded by writers, commonly of science fiction, from an early age. McLaren grew up between London's West End near Hyde Park and Bodnant, her family's 80-acre estate in North Wales. Her mother, Christabel Mary Melville MacNaghten, from London, was the daughter of Sir Melville MacNaghten, the head of Central Intelligence (CID). Christabel was one of London's most celebrated hostesses and entertained a social circle of literary figures, including the Sitwells, Virginia Woolf and H.G. Wells. Growing up, McLaren would go for long walks with Wells in North Wales, discussing, her daughter Susan Michie claims, mainly science and socialism (personal correspondence, 11 August, 2019). These figures were also scientists and socialists, and their careers exemplified the outcomes of a close nexus between science and literary writing. McLaren stayed with Wells' progressive Quaker cousins in Oxford while studying for her entrance exams and carried forward a sense in which crossing genres and platforms was important to spreading and developing scientific ideas.

Later in life, McLaren came into contact with Naomi Mitchison, a scientist, novelist in a range of genres from science fiction to historical fiction, poet, socialist, feminist, and sister and scientific colleague of the embryologist and socialist, J.B.S. Haldane. She and

McLaren remained good friends, corresponding into Mitchison's old age on issues such as world politics, women's history and plants. The letter in figure 11, for example, taken from the Anne McLaren papers at the British Library (Mitchison, 10 July, undated), reveals how they exchanged ideas about the intersections of socialism and science, the framing of socialist issues, and their own political activities.



from Naomi Mitchison CARRADALE HOUSE, CARRADALE, CAMPBELTOWN, ARGYLL. Carradale 234

July 10th

Dear Anne

Thankyou so much for the xeroxes (is that the right plural?) Tim has been reading the peculiar article and was a bit sniffy, saying JBS had not been sufficiently in touch with recent work . . Tim is off to America as a post--graduate for four years. I shall miss him a lot. Meanwhile he has decided to put a streak of peroxide through his hair and he is already over 6 feet. But there . . What I now want to find is a publisher who will pay Grace enough for say, half a year to work on the total papers. He would need that but I think it should be three or four thousand pounds and although publishers willingly pay that for certain kinds of book which seem succeed under present day capit--alism, I am not sure that they would for the sort of book that would result from this.

Oddly enough, I have been writing a piece about my father with a lot more social background but saying how I thought some of the science fitted in. I think Jack, in filial piety somewhat over emphasised our father's left-wing tendencies; I doubt if he would have been a Marxist, even if he had dropped all his pseudo-religious beliefs, But who knows? I managed to do a lecture on Marxism, with scarcely any reference to unseemly politics, but a lot about things like quantity altering quality, which I always feel is aesthetically and socially of such extraordinary importance. Oh dear, I wish I had less to do and more time to write. Yes, I will ring you up next time I am in London. But hope you may make it to her. Than an auriculatum hybrid covered with buds! Love Naomi

Figure 11. Letter from Naomi Mitchison to Anne McLaren, 10th July, year missing, British Library, Add MS 89202/4/38. The letter discusses the socialist politics of her father, J.B.S. Haldane, and describes the lectures of socialist issues Mitchison herself was giving. The letters offer insight into the kinds of conversations and ideas McLaren encountered through her lifelong relationships with socialist writers and scientists. Open Government Licence (OGL).

Mitchison also interestingly dedicated her science fiction novel, *Memoirs of a Spacewoman* (1962), to McLaren. This series of episodes from the expeditions of its narrator, Mary, sketches a world in which knowledge of the world is gained through exploration, which in turn relies predominantly on the faculty of 'communication'. Communication in the novel is about establishing a working relationship with the object of study. As opposed to the models of scientific inquiry that aim to explain objective reality using the source language of the scientist, in this model, language is itself the enabling negotiation of knowledge, emphasising the limits of the explorer's own subjectivity as they attempt to express the unknown. Mary describes the difficulties of this process:

> You've got to think yourself behind that mouth. This was quite a difficult exercise, but of course necessary before communication could be properly established ... Thinking oneself into the shape of one's contact was elementary when considering communication techniques, but sometimes one had to be very careful to think oneself back.
>
> Mitchison, 1962, p. 27

In Mary's world, language cannot be transplanted literally between individuals and domains because it is defined by its context, and this positionality is non-trivial as it is co-constitutive with our understanding of the world. Communication, the means by which we understand anything at all, is therefore inherently imperfect – it represents a stab in the dark using the means that we have as we work towards a space in which me might be able to allow our working model of reality to be changed by other languages representing other perspectives, ways of knowing, and ways of being. Mitchison seems to have recognised as early as 1962 how this model might pertain to McLaren's developing scientific approach. McLaren's interactions with Mitchison, then, offer an insight into how McLaren's personal relationships throughout her life shaped a context in which discussions about the ideologies of science were expressed creatively across genres. Mitchison's account also offers another sense in which McLaren resisted scientific convention – moving not only between disciplines but also between genres – as she developed her understanding of her science.

Circles, models and progress

McLaren, as we have seen in her 1989 WHO speech, and as was inferred by a reading of Mitchison's text, saw communication as central to negotiating the relationship between science and society. Language, for McLaren, served as a vehicle for deliberation in order to empower the public to make choices between clinical applications, and thereby shape

research agendas. McLaren reiterates her understanding of the role of public engagement in science in a 2007 article, one of the last she wrote before she passed away, and a pertinent summary of the approach she had taken throughout her career, which was published in the commentary section of the journal *Cell Stem Cell*. Under the heading, 'The Scientist's Role', she writes,

> Scientists are not ethicists: in my view, they have as little or as much right to their opinions on the ethics of human ES cell research as any other citizen. However, they do have more knowledge. They therefore have an ethical duty to explain to people what research they are doing and what the possible implications are for society. ... It is a mistake, however, to assume that the more people understand about a scientific project and its aims, the less likely they must be to reject it. There is some evidence to the contrary. If an informed public rejects some line of scientific research, we should take heed: there may be matters of social concern more cogent than just the ''yuck'' factor. Education of the public is not enough. Of course, the public understanding of science is often woefully inadequate, but the scientists' understanding of the public is often not much better. Let us aim for an informed dialog and let us hope that the media will do their best to make sure that nothing is ''lost in translation.''
>
> McLaren, *Cell Stem Cell*, 2007, p. 26

We see a direct statement here of McLaren's commitment to a dialogue based on an exchange of information expressed in resolutions that incorporate scientists' and the broader public's concerns. Science, in her view, is a tool that connects scientists to a broader constituency, not only to professionals and the elite. Communication for McLaren was about more than filling a deficit in knowledge among a scientific or governing elite, but about drawing new perspectives into her orbit.

Here we might turn to some notes that McLaren wrote in her diary following her attendance at a meeting of the Science Group of Oxford Student Branch Communist Party on 25 November 1948. Despite her liberal and socialist family background, and the expansive network of socialist writers and scientists that she was exposed to from a young age, it was not until her undergraduate degree at Oxford University that McLaren can be seen to become politically active. At Oxford, McLaren, moved in circles of mostly men, who had seen the desperate period of the 1930s and the terrible poverty of the working classes. Many of them had joined the Communist Party, and considered themselves socialists, particularly the group around Cambridge, including figures such as the organic chemist J.D. Bernal, who supervised Nobel prize winner Dorothy Hodgkin, and J.B.S. Haldane, a biologist and McLaren's supervisor on a research project on mite infestation of

Drosophila. These were men from privileged backgrounds, many of whom had been spokespeople for socialist science within and beyond the professional scientific community. McLaren, born in 1927, and having been at Oxford for the period between 1944 and 1952, entered this scene later, but also at a very distinctive time in history as the post-war influx of ex-servicemen, who had been demobilised, breathed fresh life into old-fashioned institutions like Oxford. In her recollections of this era, McLaren described everyone as having been "a bit shaken up in their ideas" with the consequent emergence of a "vibrant, open atmosphere of social and intellectual exchange" (Franklin, 2007, p. 855), and ripe for her early explorations into the intersections between socialism and science.

In one entry of the 1948 diary that I found among material not yet deposited at the British Library and held at the time of writing at McLaren's daughter Susan Michie's house, McLaren had drafted a speech, in which she aimed to clarify her thinking about "the position that science holds in the present conflict between Capitalism and Socialism" (McLaren, 1948, unarchived document, p. 1). McLaren outlines what she categorises as the "philosophical, ethical, social and 'economic" attacks that Capitalism inflicts on science, stemming from the threat that is felt by Capitalism, recognising that science is always on side of "progressive social forces" (ibid, p. 9). The attack, she goes on, is not just on "this or that isolated aspect of science, Darwinism, or Psychology, or Sociology, or anything else, but an assault against the whole rationale, the whole methodology, the very basis of Science" (p. 8).

Marxism, in her view, is also a science; it is, therefore, only understandable, she claims, that "the chief brunt of this attack is directed against Marxism – naturally enough, that is, because Marxism is the science of sciences, the scientific study of that most complex of all organised systems, human society, the science, in fact, of working class power" (ibid). She goes on:

> Marxism links together all the sciences, showing them all in their interrelation, as having a common scientific method and tending towards a single, unified, world view, materialist, dialectical; a means not only of understanding the world around us but also – and this is what the Capitalists object to – of changing the world around us … it is the task of Marxism to fight for the freedom of science, freedom in theory and freedom in practice, freedom to discover and by so doing to change the world.

McLaren diaries, 1948, unarchived document, p. 9. Reproduced with kind permission of Susan Michie

The passage shows how science, for McLaren, at this early stage in her career, is

already coextensive with her Marxism. In this sense, McLaren posits science not only as a means of understanding, but as a tool for social change. This latter function emerges from a Marxist imperative to show the interrelatedness of all science. This translates into an imperative to theorise freely, linking discovery to the project of demonstrating the interrelatedness of the sciences, which McLaren does by finding analogies that model the relationships between domains. This practical approach to demonstrating through working models is, then, very much connected to McLaren's Marxist views, and again, shows how the work of finding languages, the textual analogue for her work of making scientific models, is part of a project to show the interconnectedness of scientific problems as part of a social system, a point that only comes to light when demonstrated through analogies, which themselves are interpretations of an ongoing problematic. We see here how McLaren upholds a systems way of thinking, in line with Waddington's and the organicists, but also, importantly, how this suggests a role for the scientist as actively advocating the freedom of not only scientific practice, but also theory. This call for open theorising emerges from a duality in the way that McLaren casts science as not only a means of 'understanding', an epistemological tool for explaining, but also as a tool for 'social change'. Science can be used to spur on social change by working on scientific problems across domains. It is in this sense that we can see how scientific communication reflects McLaren's Marxist politics; it is part of the work of actively drawing on science as an approach to problem-solving based on drawing connections through analogies in a textual, interpretive mode – of using science on the path to social change.

Science, then, for McLaren, should be actively used by exposing its logic which is based on an understanding of the world as a set of organised systems. Scientific activity is interpretation centred around finding the most suitable analogy. In this speech, McLaren goes on to elaborate how this theoretical point is implemented as a pragmatic agenda:

> Marxism, indeed the whole of Science, does not claim recognition because it is based on abstract moral principles, but because it is true and because it works; and because it is true, science can be and should be used to rid humanity for ever of the evils and misery which afflict so many in the world today, and to help men and women forward to a fuller and freer way of life.

McLaren diaries, 1948, unarchived document, p. 9. Reproduced with kind permission of Susan Michie

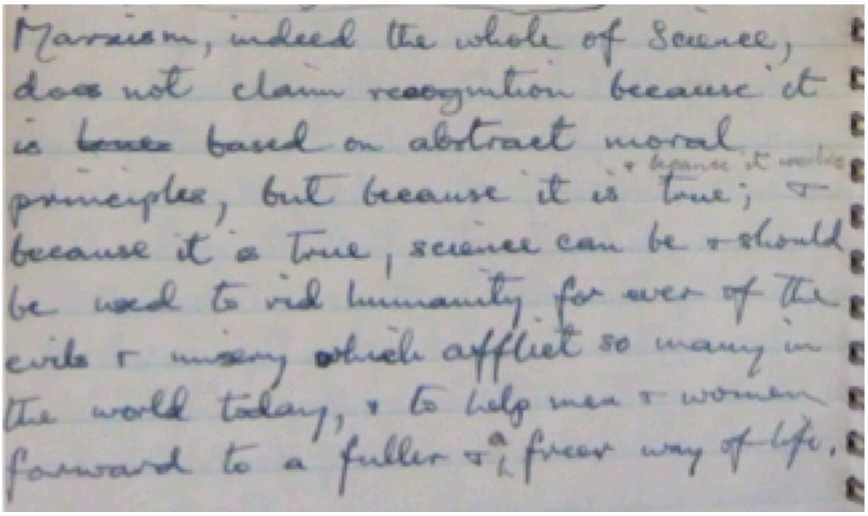


Figure 12. McLaren's diary, 1948. In this diary entry that I discovered at Susan Michie's house we see how McLaren's pragmatic problem-solving ethos is summarised in a pencilled-in afterthought.

McLaren here explains how science provides a source of ethics, not in the sense of 'abstract moral principles' but as a result of its truth, by which she really seems to mean its efficacy – the fact that science 'works'. This pragmatic justification, pencilled into her speech as an afterthought, as shown in figure 12, pre-empts McLaren's problem-solving ethos as described in her 1989 WHO speech, and throughout the Warnock debates. By 1989, it became evident to McLaren that making science work for the social good, drawing on its potential as a tool for social change, also necessitates that there is a shared language through which people are able to influence, assess and choose in the ongoing process of developing scientific applications.

This prescribes a more specific role for the scientist in public debate, as one of superimposing and integrating, facilitating and orchestrating in order to make science work. This approach now appears as a manifestation of McLaren's imperative expressed as early as 1948, to implement science by finding analogies that bring to light its interconnectedness in the Marxist social machine. Science is not about discovery, but about interpretation and explication through language. To McLaren, scientific theorising, public engagement, and laboratory science are all part of the work of doing science. This is an approach that is pervasively pragmatic, making do with the political, social and scientific possibilities to establish a tenable common ground to facilitate social progress. This process relies on expansion by connecting to new people and ideas. It is about moving freely across domains, drawing them into a model that expresses the truth of their interrelatedness.

McLaren' commitment to both science and socialism continued throughout her life.

As Andrew Murray (2007), McLaren's son-in-law, writes in his obituary for McLaren and her former husband, Donald Michie,

> They integrated scientific inquiry with the struggle for social justice – one without the other would have made no sense to them. As they sought to enlarge scientific knowledge, so also they worked to change society to allow that science to flower for the universal good.
>
> Murray, The Guardian, 2007

McLaren's political affiliation continued to be reflected in multiple ways in which she practiced her science throughout her life. Indeed, it is this commitment beyond the academic vestiges of Oxford that makes McLaren's communism so noteworthy. Communism fit relatively well with the political climate at Oxford, and, as the British sociologist and Marxist Hilary Rose, also a friend of McLaren's, put it in an interview conducted in 2018 for this study, the Communist Party was, "in a sense, the 'right place to be' in that moment in history" (Hilary Rose, interview with author, 12 September 2018). It is later in life, after McLaren left this academic atmosphere and entered into labs and policy-circles, that this commitment becomes more maverick. She is remembered for wearing plain, casual clothes to important meetings, for carrying her notes around in plastic carrier bags, and for leaving her house bare despite her wealth. Moreover, her daughter, Susan Michie, recalls how she had a practical approach to campaigning in which she drew in as many supporters as possible, across social classes. She sold papers on street corners, distributing leaflets around council estates, and every general election spent the day, along with Susan, supporting the Labour Party by driving people from the very poorest parts of Edinburgh to the London polling station (personal correspondence, 2019). McLaren's Oxford background, then, goes some way towards outlining some of her intellectual influences, but she is more remarkable for her departure from this background. It was her commitment to breaking generatively with norms – of disciplines, of social class, political affiliation and of genres – towards practical goals that was so characteristic of McLaren's practice, and reveals again her commitment to communication across domains to move her agenda forward.

A brief look at McLaren's engagement with Soviet science helps to show how much Marxism and science were entwined for McLaren, and how science provided for her a theoretical language that was to be used to communicate in order to expand the borders of the discipline – advancing her science by building alliances around theoretical models,

doing the social-scientific work of deepening her knowledge of a system.

McLaren and Soviet science

Figure 13. Meeting with Trofim Lysenko, 1957. McLaren and Donald Michie (right), Lysenko (third from left). Reproduced with kind permission of the Gurdon Institute.

In 1957, McLaren and her then-husband Donald Michie, travelled to Eastern Europe to attend the 6th World Festival of Youth & Students in Moscow, between 28 July and 11 August. Here, they met with Trofim Lysenko (1898–1976). The story of Lysenko has been rehearsed many times in the context of the history of socialism and science in the West (see for example: Peterson, 2016; Rose and Rose, 1976; Werksey, 1978). Lysenkoism was the doctrine associated with the Soviet agronomist and biologist whose interest in agriculture led him to propose that crop yields could be improved through the effects of the temperature of the seed. He has thereby become associated with the theory of the inheritance of acquired characteristics, the hypothesis that an organism can pass on characteristics that is has acquired during its lifetime to its offspring. This theory became increasingly opposed to genetics in the West on both sides of the Iron Curtain. Lysenko rose to power under Stalin and prosecuted those who adhered to Mendelian-Morganist-Weismannist genetics that posited that there could be no reciprocal influence of the soma on the heritable substance of the germ line. As Lysenko's corruption under the Stalinist regime came increasingly to light, he became symbolic of the terrors of Soviet science in the West, and the great controversy that numerous historians have credited with the dissolution of a credible scientific socialism in the West (Peterson 2016; Rose and Rose 1976; Werskey, 1978).

Lysenkoism, however, as Lewontin and Levins (1976) have pointed out, represented more than an "affair" or the "rise and fall" associated with "a single individual's influence" (p. 32). It was an ideological struggle that played out as a philosophical spat between the Weisman-Morgan-Mendel school of genetics in the West, and the supposed Darwinism in the East, which came to be expressed in two contrasting models of development. Geneticists posed a direct causal relationship between gene and phenotype, while, to Lysenkoists, the Weismann scheme did not explain the *process* of development, of how change occurs, and was therefore anti-materialist, in the sense that it "postulated effects without causes" (Lewontin and Levins, in Rose and Rose, 1976, p. 39). The Weismannist scheme also left an important paradox unanswered: the "one-way relation between gene and environment also emphasised the contradiction in genetics that all cells are supposed to have the same genes, yet produce different tissues" (Lewontin and Levins, 1976, p. 38). Mutations in development, according to Weismann's scheme, could only be surface changes, while the genetic material remained constant. Geneticists in the 1930s claimed that the unpredictability in genetic theory was epistemological only, that it was only the fact that the causal events were at a microscopic or molecular level and so inaccessible in practice to observation, and not interesting to the geneticist anyway, that meant that these could not be explained, but that there was nonetheless an "unbroken causal chain between parent and offspring and between mutagen and mutation" and that "for all practical purposes, mutations and segregations are chance events" (ibid., p. 39).

Lysenko won the official support of the party and ministries in 1948, as the world became increasingly divided into two camps: "Churchill announced the Cold War in his 1946 Fulton, Missouri speech. In 1947 the Cominform (Communist Information Bureau) was organised to replace the defunct Comintern, and Andrei Zhdanov put forward his thesis of the world divided into two camps." (Lewontin and Levins, 1976, p. 54). All of this ensured that intellectual contact between Lysenkoists and geneticists all but ceased. Lysenkoist work was ignored and ridiculed in the capitalist countries, while Lysenkoist scientists did not publish in journals or attend conferences outside the Soviet Union. It became untenable as a 'Western' scientist to take an interest in Lysenkoist science. "In the context of the Cold War", in the US, Lewontin and Levins write, "even the suggestion that Lysenko's work ought to be examined cost Ralph Spitzer his position as a professor of chemistry at Oregon State University" (p. 55). Exceptions included the work of Waddington, which showed "the basis of the apparent inheritance of acquired characters

through the discovery of genetic assimilation, the process whereby latent genetic differences within populations are revealed but not created by environmental treatment, and therefore become available for selection" (p. 55). In the Cold War context, Lysenkoism became more strident, politically opportunist and more reckless in its claims, moving from an early Lysenkoism, which still emphasised that modifying the heredity of organisms was not easy, to brash claims of being able to "transform wheat into rye in a single step" (ibid). Western genetic literature was consulted mainly to search for "'admissions' – admissions of the incompleteness of genetic theory" (ibid). Lewontin and Levins thus give an indication of how two philosophical paradigms in science became increasingly politicised and polarised, to each of their detriment.

The height of Lysenkoism had detrimental effects on the efforts by the scientists in Britain who had hoped to develop a socialist science. Lysenkoism escalated "at a crucial time in the development" of the "attempts by Marxist scientists in the West to grapple with the problem of the relationship between science and social structures", and "seemed to provide the acid test of the possibilities of a socialist science" (Rose and Rose, 1976, p. xxii) This, then, was disastrous for the idea of Soviet science. The period following 1948, the highpoint of Lysenkoism, marked a retreat in the Soviet Union to a "neutral ideology of science, and, in the West, a turning away of many scientists from the orthodox communist parties and even Marxism itself; "they were forced to choose between their science and their political convictions" (ibid., p. xxii).

By 1957 the worst of the Lysenkoist scandal had passed, yet McLaren's interest and openness to Lysenko's ideas reveals her scientific affiliation with organicist scientists, such as Waddington, who continued to take an interest in gene–environment interactions, despite the predominance of molecular biology at the time. This interest is expressed, as we shall see, not only in the content of the science that she actually discussed with Lysenko, but also in her push to improve the communication between Soviet scientists and the West in general. This shows her commitment to science as communication, moving between circles and across political borders in order to demonstrate the set of relations she was interested in.

McLaren, at the time of her visit, was working as a postdoc at UCL, alongside Donald Michie and John Biggers, extending her interest on the interaction between genes and environment through a series of experiments largely utilising the technology of embryo transfer and chimeric mice. This interest was very much at the centre of discussion in her

meeting with Lysenko on this visit (see figure 13). In one of her diary entries, again among the papers I reviewed at Susan Michie's house, she writes that she attended a meeting at "the Institute of Genetics"; "greeted by Nuzhdin Kosikov (yeasts)," she writes, "and the great Lysenko himself. 5-hour interview ... Nuzhdin a sad disappointment. Lysenko a great man" (McLaren diaries, 1957, unarchived papers). In her rushed notes of the meeting contained in one of her notebooks held at the British Library (figure 15), she summarises some of her discussions with Lysenko. One of these was the question of the role of genes in development. Lysenko, she writes, "does not accept an exact correlation between a particular point on a chromosome and a character and he thinks that Modern Western geneticists don't either" (ibid.). McLaren has schematised the question for Lysenko in a diagram (figure 14) that later appears to depict the Weismannist theory of development in her *Germ Cell and Soma* (1981), besides the updated schemes that posit a reciprocal influence of the soma on the germ line. The theorising in this monograph thus summarised her ongoing thinking regarding a scientific issue shrouded in political controversy. Rather than confronting this on political grounds, McLaren conducts a scientific investigation, expressed in the body of her work, that evolves not only through collaborations in the laboratory, but also with Soviet scientists. This is how McLaren fulfils her commitment to theorise 'freely' and equips herself with the insights and framework to make her scientific model work.

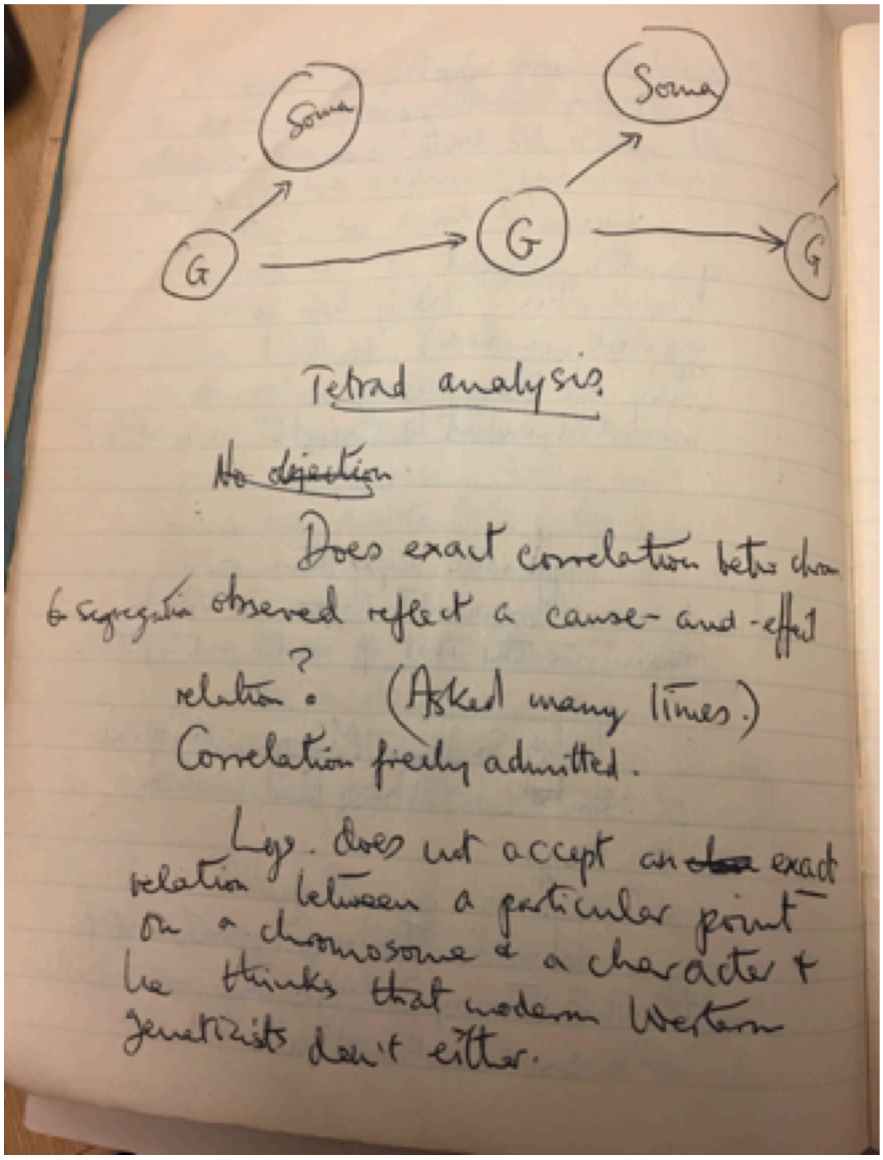


Figure 14. Page from McLaren's diaries of the trip to 6th World Festival of Youth & Students, Moscow, 28 July to 11 Aug, 1957, British Library, Add MS 89072/5/1. These notes from McLaren's notebooks reveal her developing the Weismannist scheme that later appears in *Germ Cell and Soma* (1981). McLaren is doing the work of translating the conflict in embryology about Weismann's barrier into schematic, scientific diagrams.

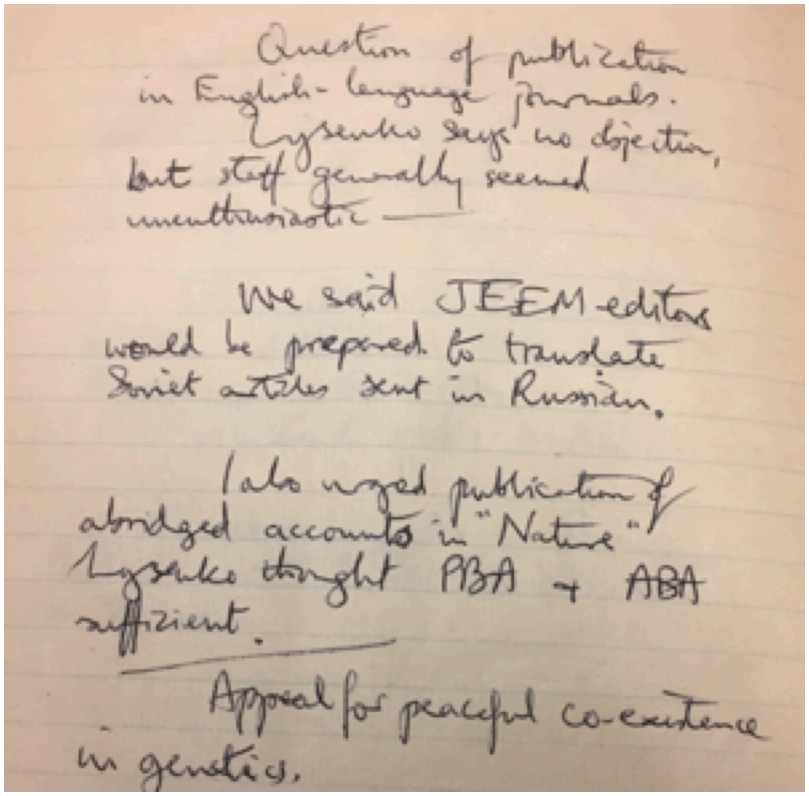
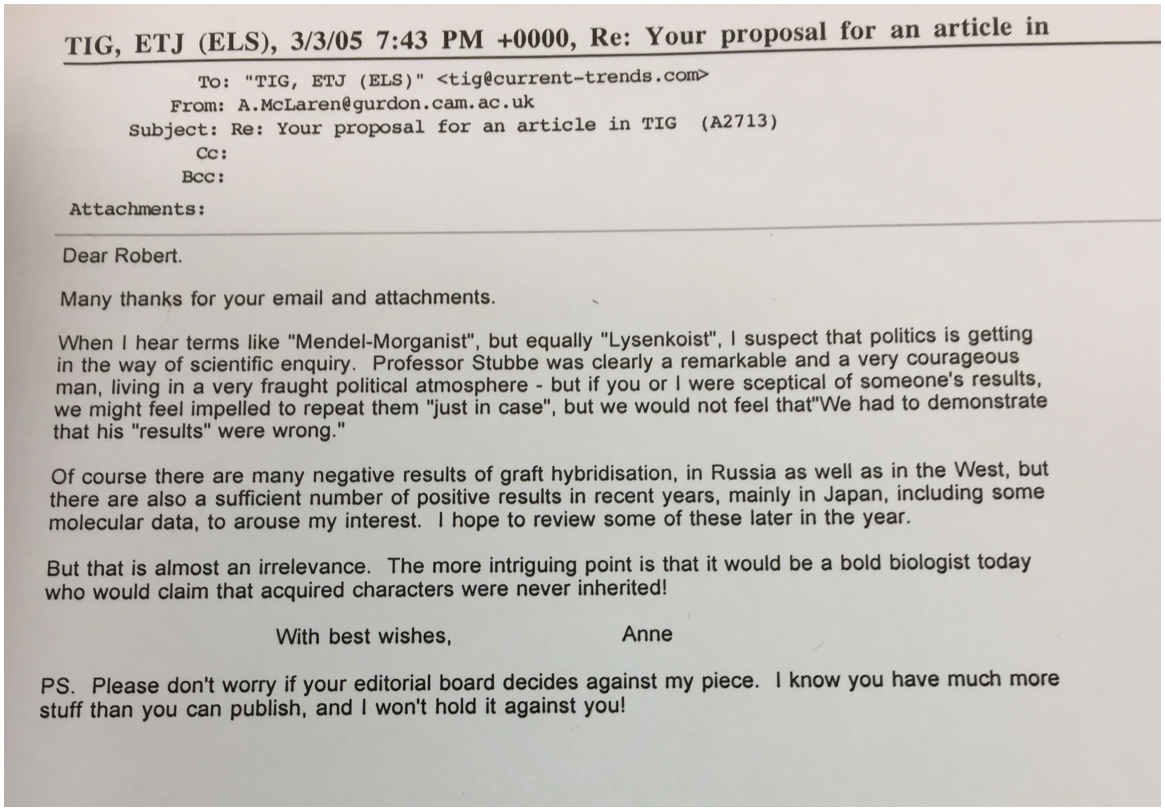


Figure 15. McLaren's notes on Lysenko meeting, 1957, British Library, Add MS 89072/5/1. These notes show the work McLaren was doing, even at this early stage in her career, of attempting to improve relations between Soviet scientists and the West.

McLaren goes on to ask Lysenko whether he would consider "publication in English-language journals", offering to "translate Soviet articles sent in Russian" and she also personally "urged publication of unabridged accounts in Nature" (figure 15), although McLaren writes that the response is "unenthusiastic" and that to the publication question, Lysenko replies that "PBA and ABA are sufficient". At the bottom of the page she writes a note: "Appeal for peaceful co-existence in genetics".

McLaren, then, sees the work of improving diplomatic relations, sharing scientific resources and theories, as necessary for the advancement of science. While her political affiliations may explain her initial interest in Soviet science, or ideas that had come to be associated with Soviet science, it is an overarching commitment to free scientific enquiry that is more broadly Marxist than the narrow politicisation of scientific research agendas. This is not the scientific determinism of the 1930s socialist scientists that posited science as the answer to social progress but, as we have seen, a commitment to scientific methods, in which she includes theorising, collaborating and connecting in order to promote the advancement of science across disciplines, as well as borders.



TIG, ETJ (ELS), 3/3/05 7:43 PM +0000, Re: Your proposal for an article in

To: "TIG, ETJ (ELS)" <tig@current-trends.com>
From: A.McLaren@gurdon.cam.ac.uk
Subject: Re: Your proposal for an article in TIG (A2713)
Cc:
Bcc:

Attachments:

Dear Robert.

Many thanks for your email and attachments.

When I hear terms like "Mendel-Morganist", but equally "Lysenkoist", I suspect that politics is getting in the way of scientific enquiry. Professor Stubbe was clearly a remarkable and a very courageous man, living in a very fraught political atmosphere - but if you or I were sceptical of someone's results, we might feel impelled to repeat them "just in case", but we would not feel that"We had to demonstrate that his "results" were wrong."

Of course there are many negative results of graft hybridisation, in Russia as well as in the West, but there are also a sufficient number of positive results in recent years, mainly in Japan, including some molecular data, to arouse my interest. I hope to review some of these later in the year.

But that is almost an irrelevance. The more intriguing point is that it would be a bold biologist today who would claim that acquired characters were never inherited!

With best wishes, Anne

PS. Please don't worry if your editorial board decides against my piece. I know you have much more stuff than you can publish, and I won't hold it against you!

Figure 16. Letter from Anne McLaren to Robert Shields, 2000, British Library, Add MS 89202/4/38. In this letter to the *Trends in Genetics* editor at Elsevier Science, McLaren appeals to the value of open enquiry in science to defend her article proposal on the inheritance of acquired characteristics.

Another piece of correspondence taken from the McLaren papers at the British Library, from years after McLaren's visit to Russia or the publication of *Soma and Germ Cell*, in 2000 (figure 16), provides another example of McLaren's approach to advancing research on gene–environment interactions. In this case, she makes her statement in response to a letter from Robert Shields, the *Trends in Genetics* editor at Elsevier Science, in which he rejects an article McLaren had proposed in which she cites studies reporting the 'inheritance of acquired characteristics' that is cast as an extension of the work done by Ivan Vladimirovich Michurin (1855–1935), who worked on plant hybridisation under Lenin, investigating the effects of the environment on the genotype. Among the several issues the Elsevier board had with the article, the last two are summative. Shields reports,

> Whether Michurin was right or wrong, his work does not appear to have led to anything beyond Lysenkoism. Whereas the rediscovery of Mendel fuelled the genetic revolution, the popularization of Michurin in the Soviet Union did not lead to any of the work that she describes as indicating inheritance of acquired traits. It strikes me as a distortion to promote Michurin's work as a prelude to later studies that indicate non-Mendelian inheritance. … Fig. 9 shows pictures of Mendel, Lysenko and Michurin. It's hard to know why, except to make some socio-political point, as opposed to a scientific one.

Shields, 3 March 2005, British Library, Add MS 89202/4/38

To this letter, McLaren responds,

> When I hear terms like "Mendel-Morganist", but equally "Lysenkoist", I suspect that politics is getting in the way of scientific enquiry … if you or I were sceptical of someone's results, we might feel impelled to repeat them "just in case", but we would not feel that "we had to demonstrate that his 'results' were wrong."
>
> McLaren, 16 March 2005, British Library, Add MS 89202/4/38

McLaren casts the rejection as a politically motivated one but defends the 'Soviet' point of view scientifically – arguing for the scientific principle of open enquiry, and then also pointing to evidence of the underrepresented view in the form of recent studies, "mainly in Japan" (ibid). She ends the letter on a light-hearted note: "PS. Please don't worry if your editorial board decides against my piece. I know you have much more stuff than you can publish, and I won't hold it against you!" (ibid). McLaren, once again in a non-divisive, non-vitriolic manner, recasts the debate around the inheritance of acquired characterises as a scientific one, revealing her ongoing commitment to inclusiveness that makes the advancement of science a tool for political progress – cutting across political divides in the shared scientific project. As in the embryology debates, we see here and in the account of McLaren's meeting with Lysenko how science, for her, provides a language that can be used to incorporate conflicting views to work towards a resolution. Science provides a solution, not in a deterministic sense, but in the form of an authoritative language that facilitates exchange across divide, and not by reconciling difference, but by incorporating the insights from all sides into a working model.

This commitment to bringing together theory and praxis for the enhancement of both, a commitment that, as McLaren's daughter Susan Michie pointed out, might also be described as Marxist (personal correspondence, 11 August, 2019), becomes especially clear in the following example in which McLaren proposes a scientific model, building on these debates about non-Mendelian inheritance, in her monograph *Soma and Germ Cell* (1981). Here, she outlines a working model that, in time, comes to be consolidated by research that, within the framing she has architected, presents evidence for her system of relations. Scientific theorising, over the course of time, is a tool for scientific progress.

Germ Cell and Soma

Here it is relevant to turn briefly to a monograph that exemplifies McLaren's commitment

to theorising in order to harness scientific progress. McLaren's *Germ Cell and Soma* (1981) can be seen as an intervention in the philosophical, politicised dispute between Lysenkoists and Weismannist-Mendelist-Morganists. Her contribution is here to provide a framework that reduces the debate to its bare epistemological scheme, detailing the research that has and could support or refute each model. The book, which is unusually "philosophical" for a scientific text (Simpson, interview with author, 21 June 2018), is an expression of a political project that, rather than championing Lysenkoism or Western genetics, seeks to facilitate exchange between them. Once again, McLaren performs her role as an orchestrator and connector, drawing disparate views together in order to explore how they overlap, and can come to overlap, in a shared research agenda. I will briefly summarise her argument below.

In *Germ Cell and Soma: A New Look at an Old Problem* (1981), McLaren tackles the longstanding question of the relationship between the somatic cells and the germ cells in the developing embryo. This had been a contentious issue in the history of embryology. Dogma in the field, especially over the 1960s, had come to dictate that there could be no reciprocal influence of the soma on the germ line. This moreover, had become a politicised issue given that the reciprocal influence of the germ line on the soma had come to be associated with Lysenko's science under Stalin's regime, as we have seen. McLaren, however, approaches the issue as a scientific one. To her, the issue is of particular interest because she sees it as another articulation of the 'nature-nurture' problem, the relationship between gene and environment, which she is interested in. As she explains writing five years earlier, in her *Mammalian Chimeras* (McLaren, 1976),

> When we consider the development of individual cells, we are faced with a more sophisticated extension of the nature-nurture problem, since the environment in which a cell develops is made up of the population of other cells surrounding it.
>
> McLaren, 1976, p. 6

In *Soma and Germ Cell,* McLaren deploys her characteristic style to frame the discussion by offering a generalizable set of models, then proceeds to detail research that exemplifies the interactions between soma and germ cell – analogies for the relationship that she is interested in, which she finally assimilates in reference to the opening diagrams, thereby fleshing out her developmental picture.

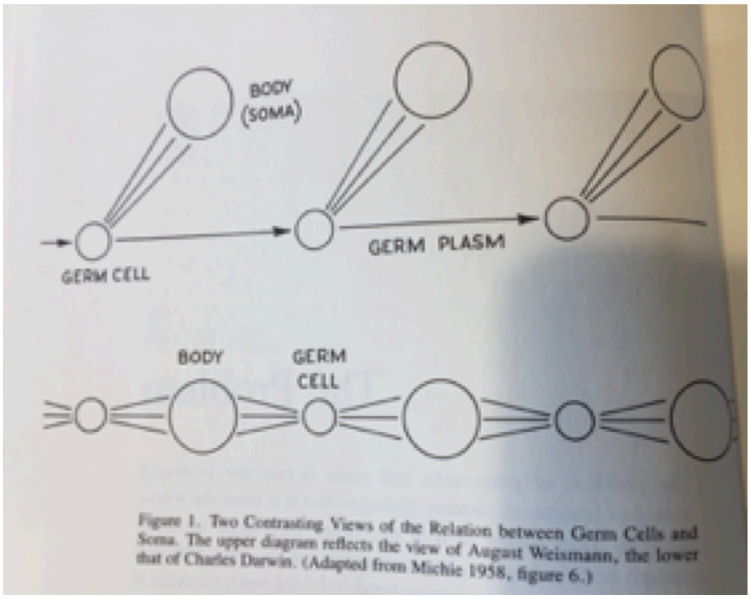


Figure 1. Two Contrasting Views of the Relation between Germ Cells and Soma. The upper diagram reflects the view of August Weismann, the lower that of Charles Darwin. (Adapted from Michie 1958, figure 6.)

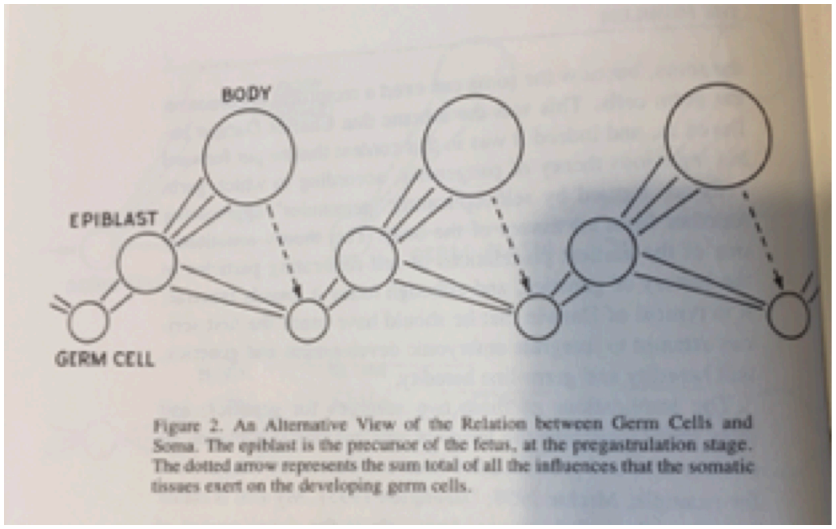


Figure 2. An Alternative View of the Relation between Germ Cells and Soma. The epiblast is the precursor of the fetus, at the pregastrulation stage. The dotted arrow represents the sum total of all the influences that the somatic tissues exert on the developing germ cells.

Figures 17 and 18. Diagrams from *Germ Cell and Soma*, 1981. Here, McLaren schematises the two contrasting views of the relationship between germ cells and soma in the history of embryology, building on the theorising she and Donald Michie had done as early as 1957 in conversation with Trofim Lysenko in Russia.

McLaren opens the book with the diagrams in figures 17 and 18. The first, she explains, is the one "associated with August Weismann" and "shows his hypothesis of a continuous stream of immortal germ plasm, concentrated at intervals in actual germ cells, that buds off to form mortal soma in each generation. Influences on the germ plasm can affect soma, but there is not – and in principle there cannot be – any reciprocal influence of the soma on the germ plasm" (McLaren, 1981, p. 3). In the second diagram, in contrast, "influences on the germ cells can still affect the soma, but now the soma can exert reciprocal influence on the germ cells", which she says, " was the scheme that Charles Darwin believed in", and in the final figure, "which corresponds more closely to

developmental reality, it is the epiblast that is the business part of the embryo, after it has formed its extraembryonic membranes but before it has started to form any organs" (ibid). The dotted arrows in this diagram represent the "hypothetical influence of the soma on the developing germ cells" and in "its philosophical implications it is closer to Darwin's scheme." (ibid). Having established these broad schemas, she goes on summaries the research being done on each part of the developmental cycle that might be used to reflect on the soma-germ cell relationship, all of which, we have seen, she believes should be considered in order to understand the relations between gene and environment. McLaren is especially interested in the development of the germ cells.

To McLaren, the embryonic development of germ cells and the question of whether or not they embark on meiosis constitutes one of the most crucial points of interaction of the germ cell with its somatic environment and illustrates well the interplay between the "environmental stimulus and the autonomous chromosomal factors that determine the germ cell's response" (p. 88). This node also embodied a central paradox, namely how germ cells can be both totipotent and differentiate into specialised forms, and it was a paradox that could not be explained using Weismann's model. In other words, the germ cell question presented a particularly apt analogy of her model system of gene–environment, here in the form of germ cell and soma interactions. McLaren goes on to point to two gaps in understanding. The first is the influence of the environment on germ cells, which she shows exists, and is the weaker version of her argument. The second part of her argument regards the origin of the germ cells. Here, she makes three conjectures: (a) the potential for totipotency rests in all cells; (b) the realisation of this totipotency rests on the cell's exposure to an adequate environment; and (c) the selection of certain germ cells to form the germ line while others form the somatic tissues depends on their position within the epiblast. If proven right, even in part, her modelled hypothesis will be deemed correct, because an "absolute distinction between germ cells and soma could no longer be made" (McLaren, 1981, p. 95). McLaren's case, then, departs by proposing a hypothetical model for development, and ends by stipulating the exact conditions under which this model must be true. She concludes,

> … if they [the conjectures] were demolished by future research, then some compromise between figure 1a [Weismann's model] and figure 2 [her model] might constitute an acceptable model. But if they were upheld, even in part, then figure 2 [McLaren's model] could be regarded as a valid updating of figure 1b [the Darwin model of pangenesis], since an absolute distinction between germ cells and soma

could no longer be made.

McLaren, 1981, p. 95

Evidence was gathered around each of these conjectures as research progressed in the field. Conjecture three, which McLaren considers "the most readily testable of the three conjectures and hence the most interesting" (p. 95), was indeed proven by one of her students, Professor Patrick Tam, who, as part of his PhD work in Anne's MRC Mammalian Development Unit on the population expansion of the primordial germ cells (PGCs) in the mouse embryo, had established the cell engraftment and tracking technique and embryo culture protocol to test the developmental fate of graft-derived cells in a host embryo *ex vivo*. Tam (Tam, interview with author, 28 August 2018) recalls that McLaren approached him about using his techniques to perform a cell transplantation experiment to test if cells in different regions of the epiblast are fixated or 'plastic' for the PGC fate. The results of his experiments (Tam and Zhou, 1996) were consistent with McLaren's third conjecture, the hypothesis that the germ line is not pre-determined but could be specified by 'position-delimited inductive activity'.

What is significant about McLaren's argument here is how it reveals, once again, her aptitude for delineating a research agenda around a specific biological model, stimulating the application of techniques towards a shared, broadly defined theoretical goal across various levels and fields of biology, in order to advance a research agenda despite its political stigma. McLaren, once again, shows a translational aptitude, the ability to perform the social and scientific work of connecting in order to move her scientific research agenda forward. As her collaborator on her work into sex-determination in immunology in the 1970s and 80s, Elizabeth Simpson, put it in an interview with me,

> … Anne is putting together a framework for asking questions. And she was very good at having a comprehension of what that framework needs to take into account. So, a lot of the arguments that she's using in here are almost philosophical, rather than scientific, which makes it sort of … you need to take a step back for a second.
>
> Simpson, interview with author, 21 June 2018

Simpson's comment reveals how the work of framing and modelling that McLaren was doing was often cast as beyond the purview of science, deeming it 'philosophical'. Even the broad theoretical framing of problems, reminiscent of the work that both Waddington and McClintock did, is 'extra-scientific' for Simpson, but is once again integral to McLaren's practice. This commitment to theorising freely, however, is an

expression of McLaren's dedication to science as a political tool. She tackles a central dogma in the field, one that had come to become politicised and divisive, simply by putting her biological model to work 'scientifically'. In applying this method, then, McLaren was not interested in distinctions between domains of knowledge. Her approach was, once again, highly pragmatic. This pragmatism relies on an ability to translate between problems, to recruit actors and technologies to a research agenda. Her commitment to Marxism consolidates and helps explain her 'social' approach to science: her 'opportunistic' shifting between analogous models that requires both the social work of collaborating, the theoretical work of superimposing, as well as the scientific work of experimenting and analysing to build a theoretical, scientific model that, she trusts, will advance the state of science given that enough research is done, and that this continues to be incorporated into the working model. This example, then, adds to the sense of McLaren's commitment to science itself as a set of tools forceful enough to connect scientists across political regimes. We also see once again how her method of superimposition was also a scientific one, in the sense that science provides a language and a method that can be mobilised to incorporate new perspectives, rendering distinctions between fact and value secondary, if not irrelevant. This was an approach that derived, in part, from her understanding of science as a tool for political progress, and of the politics of establishing and maintaining relations across borders as a tool for scientific progress.

The relationship I have sketched above for McLaren between science and the social good also foregrounds two key characteristics of McLaren's style of practice of key importance to her science, policy, and political work, namely that of 'communication' and 'comparison'. These two activities are repeatedly linked in McLaren's approach as she draws new connections between problems across fields, domains and political divides. Communication, in these cases, is essential in order to recruit the relevant actors to her project, and this in turn requires comparisons in the forms of non-literal translations, analogies that make concepts speak to the concerns and methodologies of a particular field. Communication of scientific concepts in all of these cases, means considering the language of the target group in a formative sense. McLaren's dedication to communication and comparison defines her life's work and is an aspect of doing scientific work not often acknowledged within science by scientists – but McLaren enacted a commitment to both.

Reading McLaren's legacy

In the above, the idiom of 'moving in circles' has been key to my description of McLaren's style of practice. McLaren is credited by many of her former acquaintances and colleagues for her ability to move easily in any circle. The same can be said of her ability to move between scientific problems, scientific disciplines, and even political regimes. The work of communication and comparison allowed McLaren to move in and between various 'circles'. In her science, this ability is not given the same validation by her former colleagues, leading many to discredit her science, especially from the 1980s onwards. My interviewees contended, for example, that her involvement in Warnock in 1980s marked a shift of interest that meant she was "past her prime scientifically" (Goodfellow, interview with author, 21 September 2018). In my interview with Richard Gardner (27 September 2018), evoking comparisons with the English geneticist Mary Lyon, best known for her discovery of x-inactivation which was also of major importance to McLaren's work, Gardner claims that Lyon was in a "very different league" as a "source of original ideas" to McLaren (ibid). McLaren, in contrast, he contends, was "a tremendous absorber of information," and "picked up ideas at a very early stage and developed them very competently", but this lack of originality, he implies through the comparison, makes her less of a scientist in her own right, not of "Nobel standing" (ibid). McLaren's opportunistic, broad approach to science, then, seems to have diverged too far from the conventional career trajectory for her to be considered a scientific 'great'.

I have shown, however, how communication was key to McLaren's development of her gene–environment model. Her movement between fields and problems led her to constitute a unique field of study encompassing the entire life-course of the embryo from one generation to the next. This circularity is more than a superficial point because it manifests the outcome of the ethos of problem solving that I describe in this dissertation, that relies on analogies and connection. The outcome of an approach that draws on a variety of fields to solve the social problem of investigating a scientific problem, it appears, is not an incoherent sprawl, but a coherent, meticulous model based on multi-faceted evidence. The body of McLaren's work represents an argument for epigenetic effects at various levels of biological organisation and has laid the foundation for much of the revival of epigenetics today, as active scientists in the field still confirmed to me in our interviews (e.g. Surani, interview with author, 22 August 2018). The idiom of moving in circles foregrounds just how distinctive McLaren's approach to science was and, in doing so, helps elucidate her

ambivalent legacy, which is tainted by a sense of misunderstanding and which, much like the legacy of the 'pre-embryo', needs to be explained as a failure to adhere to dominant scientific literalist paradigms.

The meaning of McLaren's scientific work, as her life, only emerges from a broad, encompassing description of the way in which she translated problems across fields throughout her life and thereby falls outside of a scientific convention in which key discoveries are explained in the language of a single field. Her contributions to uncovering the mechanism of epigenetic effects and developing the method to deduce these, is discredited, while her science was repeatedly cast as primarily 'reproductive' biology, or, as we have seen, not of 'Nobel standard' by my interviewees. In these accounts, something in her distinctive and pragmatic approach of crossing between fields, between problems and applying herself so fully to policy-work serves to discredit her professionally. I hope to have shown how McLaren's science depended through and through on a broad, integrative view, in which data was gathered from across disciplinary fields, national borders and iron curtains. Her commitment to policy and internationalism also reveals also how deep her commitment to science as a social project ran, how she saw the work of developing a social infrastructure to support her scientific aims as an essential part of facilitating science for the social good. McLaren was interested in everything that gets the individual from 'one generation to the next', and with this comes a commitment to finding appropriate analogies for the dynamic that is the red-thread in all of her work, 'gene–environment interactions'. It is perhaps this analogous, non-literalist language and method that has failed to register within the conventions of academic science even today, that has failed, to evoke Evelyn-Fox Keller's, to 'persuade'.

Thatcher, McLaren, and 'choice'

I now return briefly to the human embryology debates of the 1980s. The account I have given of McLaren's vision for science and society, of her socialism, and of her commitment to moving between circles, provides a useful background to further situate her and her approach in these public debates on science. In particular, it builds on my critique of Duncan Wilson's (2014; Chadwick and Wilson, 2018) assertion that Warnock and Thatcher shared a common anti-expert sentiment, showing how their supposed shared emphasis on individual choice held by Warnock and Thatcher were actually radically different. McLaren's style of practice helps illuminate how the language of science allowed these

figures to see eye-to-eye during the debates, but how this relied on – far from a common neoliberal sentiment – an approach that Susan Michie has pointed out, might be described as Marxist, namely a "commitment to working with the broadest possible alliance of people whose interests can bring together in support of a cause" (personal correspondence, 2019).

Wilson (2014), in his attempt to construct a distinctly British history of Bioethics, describes a shift in Britain towards external regulation of biotechnologies in which there were no moral experts, only 'different opinions' that would be factored into a process of 'corporate decision-making' (p. 141). He claims that this shift was facilitated by a conducive political environment, in that it "dovetailed with the neo-liberal emphasis on individual autonomy and echoed Margaret Thatcher's belief that 'choice is the essence of ethics'" (p. 156). As we saw in Chapter 1, Wilson argues that the Warnock Committee could thereby contribute to a Conservative effort to break the welfare-state form of government in which professional expertise was vital to the development of public policy. The Conservatives instead argued that policy should be shaped by 'outsiders' who functioned as proxies for different stakeholders and consumer interests. Warnock echoed this belief by arguing that declining trust in professional expertise transformed what were once "'matters of professional behaviour' into 'questions of public policy', which merit public discussion and therefore, because we are a democratic society, ultimate discussion in Parliament" (Warnock cited in Wilson, p. 156).

This argument becomes difficult to accept or, at least, is somewhat subsidiary, given the role that the Warnock model actually came to play in the debates, as we have seen in my account of how McLaren carried it forward. This was an iterative process of building common ground, by superimposing a social vision on the biological, legal and the ethical.

Both McLaren and Warnock, and the pro-research lobby that surrounded them, as we have seen, were very much oriented towards what Warnock herself described as the idea of the good:

> In recommending legislation, then, we are recommending a kind of society that we can, all of us, praise and admire, even if, in detail, we may individually wish that it were different. The task, one philosopher noted, was to articulate "a steady and general point of view".
>
> Warnock, 1985, p. 1

The 14-day limit that they recommended, then, stood for an idea of social morality itself. This view diverges considerably from the typical Thatcherite position in which there

was no such thing as society; Thatcher was, indeed, adamant that there was "no such thing as society" (Margaret Thatcher, interviewed by Douglas Keay, *Woman's Own*, 1987). In Thatcher's view, there were only individual men and women and their families, and the concepts of 'social order', 'social consensus', or a 'social contract' would have been of little relevance to her. Morality, to Thatcher, was a deeply individualistic affair, a legacy of Thatcherism that Warnock has lamented. She writes in her 2000 memoir,

> The legacy of Margaret Thatcher, then, is still pervasive and harmful to society as a whole. The idea of the common good, which genuinely lay behind the welfarism of the 1940s and 1950s, has simply got lost.
>
> Warnock, 2000, p. 196

Warnock makes clear, that she and Thatcher had fundamentally opposing visions of social morality. In a 1979 conference speech, Wilson points out that Thatcher had argued that "morality is personal" and denied there "was such a thing as a collective conscience" (Thatcher, cited in Wilson, 2014, p. 170).

If, then, we accept that Warnock and Thatcher could align over an understanding of Warnock's practical role as a philosopher in the debates; as one of exposing logic of arguments, making suggestions, but not as an expert, this becomes a rather superficial point of convergence, considering just how much their social models diverged. The account I have given of McLaren's vision for science and society, particularly her conception of choice in the context of family – 'decision-making' – adds to the sense in which the supposed shared emphasis on individual choice held by Warnock and Thatcher were actually radically different. McLaren, too, emphasises the importance of choice to the future of the governance of reproduction. In various speeches given on family planning in different contexts, she repeatedly cites George Elliot in her discussions of choice. In the 1989 WHO keynote, for example, she says,

> I pointed out earlier that reproduction offers the possibility of choice, responsible choice, not just necessity. George Eliot wrote: "The strongest principle of growth lies in human choice". Every animal can reproduce itself; only human kind can choose when and how and whether to reproduce. But who is to make these choices? Sometimes they may be individual choices; but often there will be conflicting interests within the family, and the decisions will have to be family decisions, taking conflicting interests into account … Then again, families do not exist in isolation, but in social groupings … so social forces, ethical pressures, social incentives and disincentives may all influence the decisions.
>
> McLaren, in WHO Keynote, Geneva, 1989, p. 14

Her ‘principle of growth’, as we have seen, is only effective when people are provided with the necessary information to choose, and influence, the options presented to them. Clinical applications, as we have also seen in the contact of PGD, in this way come to represent a shifting horizon based on social consensus, expressed in a biological model that comprises a shared language. This is not a consumer’s demand for a product taken at face value, but a process of negotiation in relation to a stack of domains that places immense responsibility with the scientist to ensure that the discussion represents and incorporates the views of experts and non-experts alike, in order to find common ground. If this model is ‘anti-expert’, in that it overcomes the sole power of the medical profession in people’s decisions about how they want to have children, it is in the sense of it being democratic, establishing a shared space for discussion, instead of opposition.

Science as a political tool

Yet, despite their diverging social views, the Warnock model did win the support of Thatcher’s cabinet, although never openly, thus still begging the question of why the PM and her Cabinet were amenable to the unprecedented government intervention in the science of reproduction presented by the Warnock Inquiry. Moreover, in talking about McLaren’s role, particularly in pushing the pro-lobby case forward and having outlined her politics as it pertained to her science, it is relevant to ask how McLaren, too, related to a Prime Minister seemingly as far from her own politics as could be. In an interview I conducted with Chris Graham in 2018 for this study, he recalls one particular visit he made to brief Thatcher with McLaren and Richard Gardner that made clear to him McLaren’s disregard for Thatcher politically:

> Anne McLaren had come in with a skirt made out of jean-material, and not dolled up at all. Richard was in a suit. I’d borrowed my brother-in-law’s suit which was pin-striped, who was a surgeon … So we went down, and Anne sat in what she thought was Margaret Thatcher’s chair and in fact she was right. So the first thing Margaret Thatcher said was, “would you mind moving?” Anne and Thatcher hated each other. They hated each other. And just apart from politics, they were completely different ends of the spectrum. I once asked Anne, you know, what really was the basis of this dislike – I shouldn’t say hatred – and she said, “Margaret Thatcher talks at breakfast”, and she knew her from being an undergraduate, although they were at different colleges. So Anne did not get on with her at all.
>
> Graham, interview with author, 27 September 2018

Graham’s quote not only makes clear McLaren’s disregard for Thatcher herself but, once again, to the ways in which McLaren resisted social conventions. McLaren’s daughter,

Susan Michie, recalls,

> Anne hated 'society' events and refused to follow the social expectations mapped out for her by her parents – she was by nature introverted and although she liked a good party and dancing, she did not like smart, stuffy occasions. She was also much more at ease with people in her local pub or café than people from her kind of background.
>
> Susan Michie, personal correspondence, 2019

That McLaren directly corresponded with and won the Prime Minister's support, despite their political and personal differences, is testament to her commitment to moving between circles, her commitment to working with the broadest possible alliance to advance her scientific agenda and, as we shall see, her commitment to scientific advancement, which coextended with the project of social advancement that came to provide the real common ground between the Warnock approach and Thatcher's. A closer look at this correspondence, and some additional contextualisation of Thatcher's overall attitude towards science policy, suggests, as opposed to Wilson's analysis, that Thatcher converged with the pro-lobby not so much over a shared understanding of individual choice or of the role of the expert, but predominantly over a shared understanding of the value of science to society.

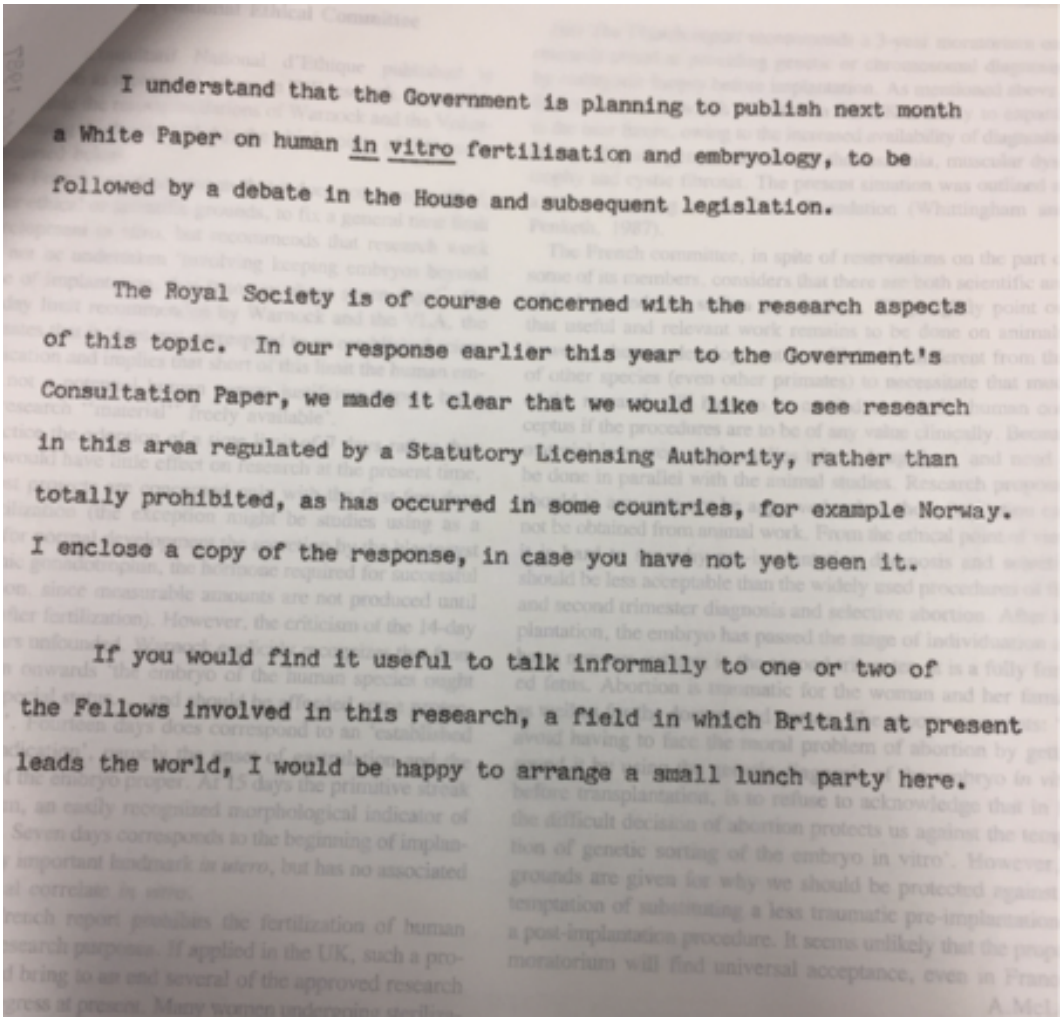
British science and technology studies scholar, Jon Agar (2011), in his article exploring the role of science in Thatcher's policy, draws on an understanding of her pre-political, scientific career, to posit that, given this experience as a "research scientist", Thatcher had a "different view of science from that of any other minister responsible for science" (Agar, 2011, p. 215). Thatcher's training as a chemist at Oxford (where she crossed paths with McLaren), her four-year dissertation in X-ray crystallography of gramicidin after World War II and, importantly, her four years' experience as a working industrial chemist at British Xylonite Plastics and at Lyons, meant, Agar argues, that science came to hold a "peculiarly significant strategic position for Thatcher" in two regards (p. 226). First, "science represented the best of the public economy, and the research councils … as places where the public economy worked". This belief emanated from a "genuine and unforced … esteem for elite scientists" (Agar, 2011, p. 226). He goes on:

> She viewed science as a source of wealth, and therefore a justified expenditure from the public purse. Yet this elevation made science even more of a test case for the developing views on economic liberalism. If markets could work for science policy, they could work anywhere.
>
> Agar, 2011, p. 226

This belief in the – marketable – value of science, then, explains how Thatcher may have been supportive of the continuation of embryo research, especially in aid of clinical applications for which there was demonstrable public support and demand. The second consequence of Thatcher's research experience to which Agar points also suggests why Thatcher may have been amenable to the setting up of a government inquiry into human fertilisation and embryology made of non-experts:

> It was precisely because Thatcher knew what scientific research was like that made her impervious to claims that science was a special case, with special features and incapable of being understood by outsiders, and therefore that science policy should be left in the hands of scientists. … Thatcher, who lived both worlds, saw no separation, in principle and in practice.
>
> Agar, 2011, p. 227

It is this background that explains both Thatcher's esteem for science, but also how it was under her that the science of reproductive technologies came under public scrutiny, as an area that should be marketed and governed like any other. It is perhaps also for this reason, that McLaren was able to appeal to Thatcher in the lead up to the 1987 White Paper vote that would decide between continued research under regulation and a ban, to discuss the pro-lobby position from a scientific perspective. McLaren can be seen here to be orchestrating the pro-lobby case once again, this time with regard to Cabinet.



I understand that the Government is planning to publish next month a White Paper on human in vitro fertilisation and embryology, to be followed by a debate in the House and subsequent legislation.

The Royal Society is of course concerned with the research aspects of this topic. In our response earlier this year to the Government's Consultation Paper, we made it clear that we would like to see research in this area regulated by a Statutory Licensing Authority, rather than totally prohibited, as has occurred in some countries, for example Norway. I enclose a copy of the response, in case you have not yet seen it.

If you would find it useful to talk informally to one or two of the Fellows involved in this research, a field in which Britain at present leads the world, I would be happy to arrange a small lunch party here.

Figure 19. Letter from McLaren to George Porter, 1987, British Library, Add MS 89202/8/3. McLaren writes to George Porter, president of the Royal Society, to call for a meeting to discuss human embryo research with Margaret Thatcher. McLaren appeals to a common understanding of the importance of science to Britain.

In figure 19, McLaren writes to George Porter, president of the Royal Society, to organise a lunch meeting with the Prime Minister to discuss research, which, she stresses in the letter, is a "field in which Britain at present leads the world". This is McLaren's attempt, therefore, to establish an alliance between the community of scientists at the Royal Society and Thatcher, over a shared interest in the role that science could play in Britain's global status – an allusion to the economic interests at stake in scientific research. The appeal seems to have worked, as Thatcher agreed to a meeting and McLaren, along with Richard Gardner and Chris Graham, were invited to visit Thatcher to brief her that year. Richard Gardner recalled the experience to me in an interview conducted in 2018:

> … I can't remember which day of the week it was but it was in the Green Room. And we were told in advance that she found it really hard to keep quiet. But she gave us each fifteen minutes without hardly any interruption at all. The only thing she couldn't get her mind around was embryo banks … apart from this hang up over

embryo banks she understood everything we said, she took it all in.

Richard Gardner, interview with author, 27 September 2018

Yet despite her apparent interest in and aptitude for the science, Thatcher's support was restrained. The developmental biologist Chris Graham also present at the meeting as a representative of the Royal Society, for example, recalls,

> … what we wanted her [Thatcher] to do was to photograph her visiting a clinic, because that would have turned the public – shown them that were was overt support from the PM, she wouldn't have had to say anything. But she refused to do that and she said "I wouldn't oppose the Committee's report going through parliament", which I guess was a step in the right direction.
>
> Chris Graham, interview with author, 27 September 2018

This sense of implicit, but reluctantly expressed, support is evident in all of the correspondence I found between McLaren and Thatcher. McLaren, maintaining her clear and broad vision of the debates, for example, in several instances appealed to Thatcher to ensure that the issue of abortion was kept very separate in the parliamentary debates.

30 March 1989

Sir George Porter PRS
President
The Royal Society
6 Carlton House Terrace
London
SW1Y 5AG

Dear George

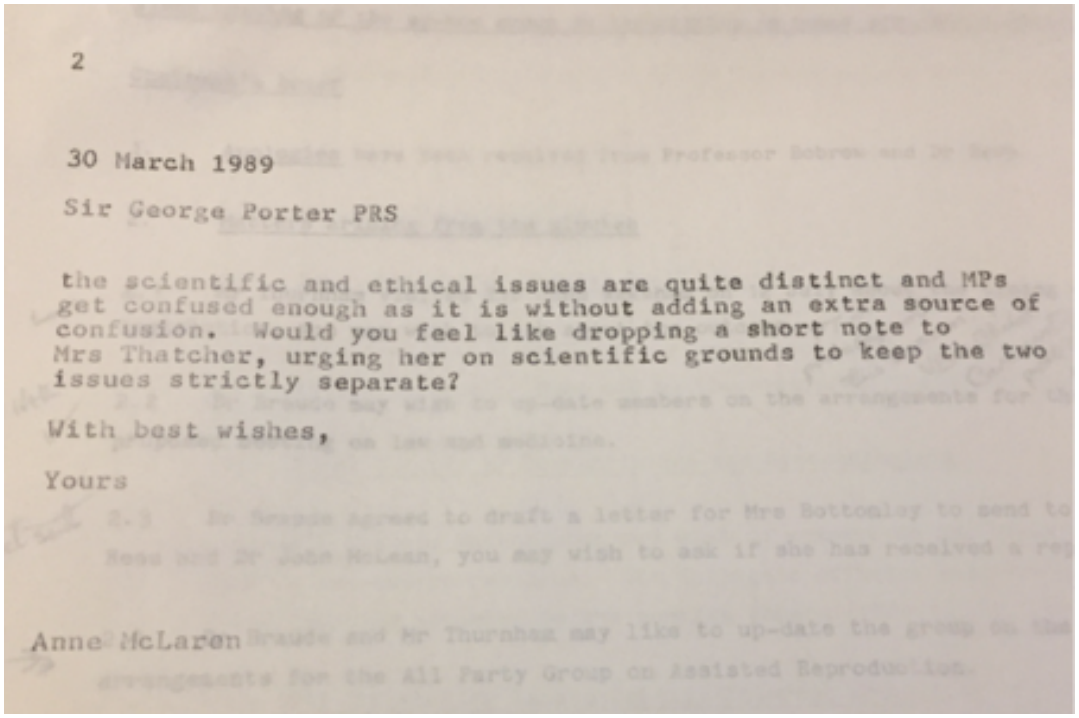
As you probably know, the Duke of Norfolk has introduced into the House of Lords an Enoch Powell-type Bill seeking to make criminal all the research at present licensed by the Voluntary Licensing Authority. The Bill went through its second reading on March 7th, without a vote: the House was packed with the Duke of Norfolk's supporters and there were many fewer speeches in support of licensed research than in the debate last year on the White Paper.

The Bill is to be considered again by the Committee of the whole House of Lords on April 26th. I wondered if it would be possible for you to write to those of the Royal Society's peers who might be interested, drawing their attention to this matter and sending them the excellent briefing material that the Society brought out before Christmas.

The Lords that occurred to me (you may know others) were;

Adrian,
Hailsham,
Halsbury,
Perry,
Rothschild,
Todd,
Zuckerman.

On a different but related topic, there was a disturbing report published in the Guardian, to the effect that the Government is planning to include the whole abortion issue in the Warnock legislation to be put before the House of Commons in the next parliamentary session. This would be most unfortunate:



2

30 March 1989

Sir George Porter PRS

the scientific and ethical issues are quite distinct and MPs get confused enough as it is without adding an extra source of confusion. Would you feel like dropping a short note to Mrs Thatcher, urging her on scientific grounds to keep the two issues strictly separate?

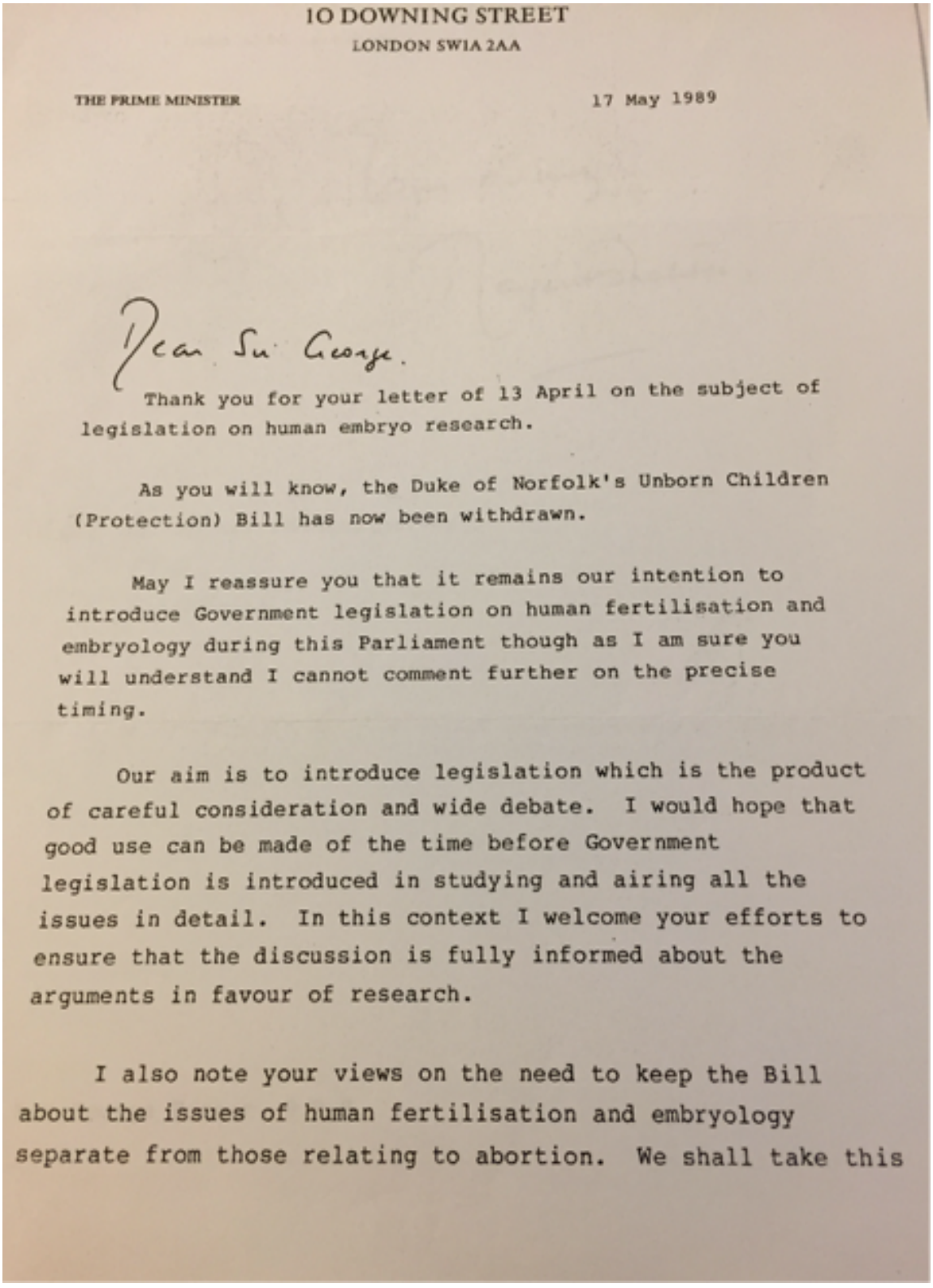
With best wishes,

Yours

Anne McLaren

Figures 20 and 21. Letter from McLaren to George Porter, 30 March 1989, British Library, Add MS 89202/9/29. In this letter, McLaren appeals to George Porter, president of the Royal Society, asking him to urge Thatcher to keep the question of abortion separate from the debates on human embryo research. The letter again reveals the extent to which McLaren relied on Thatcher's understanding of the nature of scientific research to gain her Cabinet's support.

The letter in figures 20 and 21, taken from the McLaren archives at the British Library, shows McLaren writing to George Porter again, this time to urge Thatcher to 'keep the two issues' of abortion and the Warnock legislation 'strictly separate' on 'scientific grounds'. Again, McLaren here appeals to Thatcher in her capacity as a scientist, and, therefore, in the hope that Thatcher will recognise the case for separating the issues 'scientifically'. Of course, there was a huge political interest in doing so, given the ways in which pro-life groups had attempted throughout the debates to leverage the case against human embryo research to challenge the abortion law (Mulkay, 1997). Yet McLaren chooses to appeal to Thatcher through the avenue provided by the Royal Society. Once again, Thatcher seems sympathetic to this scientific case, understanding that research is, in fact, an issue to be separated from the political issue of abortion, and responds with the reassurance that she will do as McLaren requested (figure 22).



10 DOWNING STREET
LONDON SW1A 2AA

THE PRIME MINISTER

17 May 1989

Dear Sir George.

Thank you for your letter of 13 April on the subject of legislation on human embryo research.

As you will know, the Duke of Norfolk's Unborn Children (Protection) Bill has now been withdrawn.

May I reassure you that it remains our intention to introduce Government legislation on human fertilisation and embryology during this Parliament though as I am sure you will understand I cannot comment further on the precise timing.

Our aim is to introduce legislation which is the product of careful consideration and wide debate. I would hope that good use can be made of the time before Government legislation is introduced in studying and airing all the issues in detail. In this context I welcome your efforts to ensure that the discussion is fully informed about the arguments in favour of research.

I also note your views on the need to keep the Bill about the issues of human fertilisation and embryology separate from those relating to abortion. We shall take this

Figure 22. Letter from Thatcher to George Porter, 17 May 1989, British Library, Add MS 89202/9/29. In this letter she agrees to separate the issue of research from the political issue of abortion, revealing her sympathies towards the scientists' case for human embryo research. Open Government Licence (OGL).

These glimpses of the communication that occurred between McLaren and Thatcher, however limited and often indirect, reveal that the basis of the exchanges between

them during the debates were always scientific. It is always within her capacity as a scientist and in providing information about the state of research that McLaren is able to appeal to Thatcher and influence her. This suggests that it was not so much a shared interest in empowering the consumer to choose, but a shared understanding of a specific vision of the scientific facts and the need for scientific research in aid of clinical applications – the 'value', whether economic or for the social good, that science brought to society, that allowed McLaren as a Marxist scientist and Thatcher as a Conservative Prime Minister, to find common ground.

As in Chapters 1 and 2, we see here even more clearly, how McLaren was able to exercise her scientific authority as a result of its intersection with her social class, as a member of the Royal Society, adding again, to previous descriptions of how scientific authority functioned in the debate (e.g. Mulkay, 1997). Science only reinforces her public authority, and this worked within a British political culture systemically reliant on expert trustworthiness and credibility. STS scholar Sheila Jasanoff (2005) emphasises this dimension in her cross-cultural comparison on the debates on biotechnology in democratic nations. She writes,

> Science in Britain enjoys a no less preeminent intellectual status than in the United States, but as a guarantor of political legitimacy science lacks power unless it speaks through socially accredited expert bodies, both individual and collective. Much energy is devoted in Britain to producing experts whose right to speak on behalf of the public will be virtually unquestioned. Their authority is not a matter of skills and knowledge alone, but of those attributes coupled to significant demonstrations of social responsibility. British experts generally earn the right to represent the public through successive episodes of personal testing that elevate them to the ranks of the "great and the good" and endow them, in effect, with common vision: the power to see for the people, with an encompassing gaze that goes beyond the ordinary person's capacity for foresight.
>
> Jasanoff, 2005, p. 289

Wilson overlooks this important cultural and political context in his analysis, one that is brought to light by McLaren's interactions with Thatcher, which is distinctive of the British context where legislation was negotiated, and shows the extent to which expertise was key to the negotiations that led to the HF&E Act. While it may be true that Warnock or McLaren pandered to Thatcher's notion that there should be no public experts, they nonetheless also, importantly, appealed to her belief in the value of science, as one that could and should be open to the influence of non-scientists, but that was still held in esteem, and whose representatives drew on its authority to exercise their social responsibility.

McLaren's commitment to moving across circles to draw people into her scientific orbit, through a shared and evolving biological model, exposed the interrelatedness of science and other social domains, and thereby advanced a scientific agenda geared towards social progress. This allowed her to appeal to Thatcher's understanding of science, made her credible as an authority to Thatcher, and begins to explain how the Warnock model came to be enacted in law under Thatcher's government.

Conclusion

In this chapter, I have described McLaren's approach to science, drawing on her own descriptions of her career trajectory and interviews with her former colleagues. I drew on the work of C.H. Waddington, whom McLaren has referred to as a significant influence on her thinking, to further define the epistemological approach she developed over the course of her science and policy work. I also referred to her social context, her Marxism and the legacy of 1930s socialist science in order to develop the role that public engagement played in McLaren's conception of the relationship between science and society. All this contributes to a characterisation of McLaren's style of practice, which, I have shown, might be described as a problem-solving ethos.

This pragmatic moral stance is characteristic of McLaren across all the contexts in which she was active and, indeed, is reflected in her commitment to exercising this approach in various circles, drawing them into her scientific orbit by modelling shared problems. The scientist, in this model, bears a responsibility to engage in broad deliberation, by applying concepts across domains by finding appropriate analogies in order to arrive, temporarily, at consensus. This consensus is often expressed as an agreement on a prospective clinical application – in fact, consensus is part of putting this application, pragmatically, to work. This agreement, however, is disingenuous unless the participants in debate depart from the premise that this will always be built on fractured, shifting ground. In order for consensus to be that and not imposition, we must take seriously the ongoing process of translation on which it depends – the biological, professional, affective translations that describe how perspectives are superimposed to build common ground.

Scientific practice, in this model, is centrally about communication, about modelling common ground in the form of a shared language. This style, and an understanding of the scientific, social and political commitments that underlie it, is exemplified by McLaren in the Warnock debates, as in her science. Here she exercises her

pragmatic, ‘opportunistic’ approach of leaping between groups and analogies of the same overarching model: connecting, orchestrating, mobilising and theorising, and building a translational model for getting from one generation to the next – all with an eye to the social good. The role that science plays in McLaren’s social vision, moreover, leads to a reinterpretation of Thatcher’s stance in the debates, and her relationship to Warnock and McLaren, as I argue that it was a shared commitment to the advancement of science, rather than a superficial common disdain for expertise, that allowed these key actors to unite over a common, scientific, agenda. This, in turn, consolidates my argument that science and scientific facts provide a useful, translational language through which to discuss and advance social questions and, when used reflexively, present the possibility of cutting across the stalemate of ethical and political division, towards pragmatic consensus based on what science can do for society.

Chapter 4
Lessons in public deliberation of science

In this chapter I offer a more normative contribution to the discussions around policy approaches in reproductive medicine in Britain today, as well as more specifically on the 14-day rule. I make this contribution as a response to the reluctance of some scientists, as I have documented through my interviews, to associate themselves with this era of public deliberation. This amnesia, I argue, inhibits the important contributions that scientists might make to public deliberations around science policy in Britain today. I appeal here, once again, to a broad understanding of the process of clinical translation, in order to further define the role of the scientist in public debate. I have used a plurality of translations throughout this thesis to draw attention to the legal, ethical, affective, and political work associated with finding a common biological language in the social process of arriving at a clinical translation. In this translational model, all of these aspects characterise the work of the scientist and for McLaren, too, as we have seen, these activities were intimately entwined. I hope that McLaren's model as I have described it will serve as a reminder of the work that was done by the 1980s pro-research lobby in order to arrive at legislation, and that it will incite scientists to engage openly in the process of deliberation, acknowledging that 'non-scientific' language can nonetheless be part of the process of arriving at scientific applications; that the work of fostering an environment that is conducive to research can and should involve the work of finding mechanisms to take on board the views of a wider constituency and to implement these practically.

In light of this attempt to revive some of the lessons 'not learned' from the debates in the 1980s, I turn to a conference that was geared towards finding new models to describe the relationship between science and society to guide science policy in Britain. After exploring some of the lessons that this conference teaches about the role of scientists in public debate, I will comment on the relevance of these models to the question of the 14-day rule today.

Science policy in Britain: Talking about 'translation'

In December 2017, the Wellcome Trust sponsored a 10th anniversary Anne McLaren Memorial Trust Fund Conference, *Anne McLaren and Translation*. This event brought together science, art, and the humanities in order to address the increasing role of

interdisciplinarity and dialogue in the future of biomedical translation. The conference agenda thus drew together two key parallel shifts – one was the move towards translation as the dominant model guiding science policy and funding strategies since the 1990s, aimed at accelerating innovation by removing barriers to the smooth flow of ideas in a translational 'pipeline' from 'bench to bedside', as outlined in the introduction to this dissertation. The other, similarly a shift in the funding of scientific research in professional science, was the greater attention being paid to the need for public engagement described as 'the turn to dialogue'. The centrality of dialogue to UK science policy has been traced back, in a report by Burchell, Franklin and Holden (2009), to a 2000 report *Science and Society* by the House of Lords Science and Technology Select Committee, which called for a model of science communication based on mutual exchange between scientists and the public, as opposed to a unilateral model in which scientists impart information to improve public understanding of science. The goal of the conference, as the introductory pamphlet states, was to bring these agendas together in order to "set the stage for a repurposed set of translational models that would incorporate a wider range of knowledge practices, disciplines and methodologies", thereby developing a new language of translation that is "both responsive to the need to increase the effectiveness of the innovation process, and cautious about the limits of end-directed research" (conference pamphlet, 2017).

The conference was thus framed very much in the spirit of Anne McLaren's problem-solving ethos that I have described in this thesis – the need for communication and deliberation is here used to offset and challenge the output-focused 'pipeline' model, in order to develop a more complex understanding of the process of arriving at any clinical application, but also actively searching for the ways in which this process can be understood and harnessed explicitly through active deliberation. It was interesting to see at the conference how what might be called 'conventional' fears about protecting science from the non-scientific influence of realms such as ethics and politics were side-lined through a shift in focus to the process of arriving at a clinical translation. The call for increased dialogue with the public, as well as the pressures of clinical translation, led some research scientists to make a plea to guard scientific objectivity, to ensure that scientific research remains rigorous and that information circulating in and beyond the laboratory remains accurate. As the discussions ensued, the territory to be fortified was remapped. By re-evaluating the entire pipeline process, by showing how the scientist can be cast as one stakeholder in a process of arriving at clinical translation through dialogue, the central

question became one of how to facilitate this process.

Central to the discussions was a return to the history of the HF&E debates in the 1980s as an example of the process involved in developing scientific policy around clinical translation. Anne McLaren's daughter, Susan Michie, reminded the attendees that the 14-day rule was the outcome of a long 'campaign'. Peter Braude helped elucidate what this campaign entailed. Looking back, he said, it is important to ask, 'What needed regulating in the first place?'. He referred to the work that the Warnock Committee did, outlined in this dissertation in Chapter 1, of reframing the question at hand. As we have seen, the Report did not take the question of the status of the embryo, which had defined the discussions about the permissibility of human embryo research to that point, as its point of departure, but instead asked what useful research could be done using the human embryo. It asked, in Braude's words, what needed regulating and, in this effort, looked at the research being done, the research that could be done in the future, and the purposes of this research. Increasingly, as I have shown in Chapter 2, drawing on the prospect of PGD in the debates, this became part of an effort to renegotiate what was considered experimental by positing clinical goals and showing what research was needed to ensure their efficacy and safety. The goal of a clinical translation in this model, provided a means of negotiating what I, following various scholars on the debates, refer to as a 'social contract' (Franklin and Roberts, 2006; Jasanoff and Metzler, 2018), in which scientists were afforded space for basic research in exchange for strict regulations by limiting research up to 14 days, and by ensuring its regulation under the Human Fertilisation and Embryology Authority (HFEA).

This understanding was reached through a discussion of goals that represented a shared belief in what science could do for society. Braude similarly pointed to the clinical translation of IVF, by which a formerly experimental technique became a clinical reality, a technique that was still in its infancy and to this day, as especially the senior scientists at the conference were keen to point out, remains relatively marginal with success rates much lower that popular media and advertising attests. IVF was turned from something experimental to something practical, he says, and was another form of translation. As I have done in this dissertation vis-à-vis PGD, by casting the negotiation of experimental–clinical as a translation in itself, clinical translation becomes part of a public negotiation of the techniques that they would like to see implemented and delineates an imperative for scientists to show what this will entail. It is in this sense that, as Martin Johnson said at the conference of the work done by the pro-research lobby in the 1980s, that the work of

scientists was not one of ‘persuading, but of informing’.

Lessons in dialogue

We might return here to the turn to ‘turn to dialogue’ described above, so distinctive to recent policy in British research community. As much as the turn to dialogue represented a move towards a model of public engagement that would be more participatory, and that facilitated interaction between diverse groups of science and society, this guiding programme, like translation, has raised a set of potential problems to professional science. Discussions at the conference, as well as my dissertation as whole, show how bringing translation and dialogue together as two dimensions of arriving at a clinical application, improves and directs the relationship between science and society. Both become part of doing what Charis Thompson calls, ‘good science’, a term to which I will turn after I briefly summarise the tensions expressed by scientists in response to the turn to dialogue.

The findings of Burchell et al. (2009) revealed a widespread dedication to and recognition of the validity of dialogue among the scientific community but that was coupled with a set of concerns that stemmed from a failure to formally institutionalise this activity as part of scientific professionalism. The qualitative data presented in the study reaffirmed the extent to which the two-way, reciprocal model of engagement activities as exchanges has become a dominant paradigm, or ‘ideal type’, of engagement activity in British science. Public engagement and dialogue activities were seen by the study’s interviewees to be desirable and necessary because science and the public are increasingly understood as interdependent and mutually beneficial” in that “science presents social challenges and is dependent on public funding and consent; in turn, science benefits patients and the public, while the public can benefit science” (Burchell et al., p. 75). This series of interactions was “most optimistically conceived as a ‘virtuous circle’ or ‘win-win’ situation” (ibid).

Nonetheless, the results showed, the activities of public engagement were largely regarded as extraneous to science itself, it was a “profession worthy of time and commitment; at the same time, it is an activity that is under-rewarded and potentially professionally distracting and stigmatising” (p. 75) This also leads to concerns about impact, a perceived need to measure outputs in an institution that does not value this work. This ambivalent attitude is summarised in the report:

> It is thus, confusingly for some scientists, seen to represent both the best and the worst features of scientific professionalism – at best an expression of the chief ideal

> of science as a vocation (to further scientific progress), and at worse a symptom of professional failure. The strong finding that public engagement work can be stigmatising and, in this and other ways, potentially detrimental to a professional scientific career is a reminder that the 'sea change' has also produced an undertow of tension.
>
> Burchell at al., 2009, p. 75

The study, then, reveals how the 'virtuous circle' of public engagement is utopian unless it is rooted in the institutional structure of science itself. The conference discussions as well as my findings in this dissertation, show how dialogue can be legitimised as a scientific activity by linking it to actual research programmes, through the broad aim of clinical translation. By bringing these together, we reposition the work of facilitating dialogue within the scope of scientific professionalism, in order to support a shift in the funding of research in professional science to prioritising the relationship between science and society through science policy. This framing of the relationship between translation and dialogue, as was exemplified by McLaren's approach described in this dissertation, leads me here to propose a model for public engagement centred on dialogue around clinical translation.

I propose this model by drawing on several key lessons learned from my study of McLaren's role in the HF&E debates as well as the discussions at the conference:

> **Lesson 1.** Clinical translation can serve as an orientation point in discussions about policy, as part of a process of arriving at scientific research agendas that are rigorous, while also recruiting public support for basic science. The work done by the pro-research lobby in the 1980s and the resulting legislation, shows how extensive public deliberation does not endanger basic science but, in fact, has led to a social contract that has allowed basic research in Britain to thrive. IVF, and then PGD, I have shown in this dissertation, served as a basis for delineating the research that would be required to improve the efficacy and safety of these techniques as part of the process of engendering support for these goals of clinical translations. PGD, a technique for which McLaren noticed there was widespread support and enthusiasm, based on the benefits it presented to human health, became the point of departure for a model that was iteratively developed over the course of the debates to include new perspectives and concerns. McLaren made distinctions that were incorporated into the biological model she used in public information material, such as that between selection and editing in genetic engineering, and these were based

on public consultation – on the evidence she gathered serving on committees that were meant to be representative of a lay community, but also through various public engagement events with special interest groups and mothers themselves. The clinical aim in her model of public deliberation was not definitive but was a means of exploring the values, feelings, science and legal provision that would be required to make the technique work. In discussing this, the scientific and social infrastructure required to support the clinical translation was developed. Scientific engagement in this way engenders and does not endanger the advancement of science, nor the space for scientifically rigorous and objective research.

Lesson 2. Scientists have feelings too. This leads to another point, and one that I have shown was very much relevant to the way in which the Warnock model was developed by Anne McLaren in the debates, namely that scientists can draw on a feeling of what is right, not only their objective scientific attitude, to appeal to the public. Honest exchange in this sense does not tarnish their scientific objectivity or credibility in the public eye but is a necessary precondition. This point is supported by the general acknowledgement in various studies on the public deliberation of biotechnology, that public fear surrounding scientific advances stems predominantly from a concern that unforeseen consequences, and social impact will not be considered in the advancement of science, potentially evident in the disinterestedness of scientists themselves (Taylor, 2007). This can be ameliorated by scientists' engagement with the public over palpable goals in which human health is central.

This is another advantage of the model I propose. The emphasis on clinical application as a broadly shared research agenda is that scientists are posited as feeling, interested parties, and this, once again, engenders public support for scientific research programmes. If scientists, as the public, are both stakeholders in the sense that they are working towards a set of aims, the public can be reassured that scientists are not disinterested: not only in the economic sense that they acknowledge how they are invested in a translational pipeline that has become marketized, but also in the feeling sense that they have at the heart of their practice a shared concern with human applications. Scientific objectivity does not need to spell a generalized neutrality, and this stance becomes easier to uphold when scientific information introduced into public discussion is explicitly geared towards

informing decisions on specific questions, allowing scientists to reveal their multifaceted reasoning as they think through the problem along with the general public.

Lesson 3. Feelings can be explained 'scientifically'. This positioning of clinical translation as the focus of public engagement carves out a more focused remit for scientists in public deliberation, in the form of an imperative to explain the possibilities and limitations, alternatives and required basic research surrounding a given clinical translation. When providing this information in the context of a discussion that explores clinical possibilities that the public value, and with an understanding of the broader values that these clinical translations represent to the public, scientists can engage more effectively in the debates, providing the public with the information that is most relevant to them, that speaks to these concerns. This affective understanding of what lies at the heart of a discussion can guide the scientist in expressing this understanding 'scientifically', in the form of the most relevant information to the question at hand. Relevance, in this case, is not determined only by the state in the field of science concerned, but by the dominant concerns and interests implicit in the public's views on clinical translations. This imperative gives real meaning to the call by Jim Smith, Director of Science at the Wellcome Trust, to "use science to influence policy" (email correspondence, 20 July 2019).

Lesson 4. Scientific information is a prerequisite for democratic decision-making in human fertilisation and embryology. Although it may seem redundant to claim that, to talk about science, we require scientific information, the discussions around specific clinical application reveal what is at stake in ensuring that adequate information is available to form an opinion. Without the necessary information required to discern between translational choices, the public cannot really be considered to have a 'say' in the direction of research in Britain. As Virginia Bolton put it in my interview with her, "If you're going to have a massively important vote that is going to change the course of history then you want voters to be informed" (Bolton, interview with author, 16 October 2018). A vote cannot be considered democratic unless voters have the information needed to assess the relative merits of their options. Information, then, is key to the deliberative model and scientists are indispensable to this process of engaging in an intimate sense with the

> identification of clinical possibilities and their evaluation in order to adequately inform discussions with the most relevant information possible. The key issues underlying public attitudes towards specific applications will only be ascertained through explorative discussion. The specificity of single-issue votes and the focus on developing aims in the form of clinical translation allow scientists to cater their information carefully to the vote at hand, thereby empowering the public in a real sense.

Anne McLaren writes,

> Education of the public is not enough. Of course, the public understanding of science is often woefully inadequate, but the scientists' understanding of the public is often not much better. Let us aim for an informed dialog, and let us hope that the media will do their best to make sure that nothing is 'lost in translation'.
>
> McLaren, 2007, p. 26

Dialogue will be lost in translation in more ways than one, unless it is explicitly coupled with clinical aims, and unless it is premised on a shared understanding of the information that is relevant to the question at hand.

Ethical models: Reframing the debate through clinical translation

The above call for a rhetorical emphasis on clinical translation in order to facilitate debate has the potential, if not properly facilitated, to reaffirm the current emphasis on clinical outputs in the postmillennial pipeline model in biomedicine. I here refer briefly to the work of the sociologist Charis Thompson (2013) and of several critics in feminist science studies, notably Susan Squier, in order to clarify some potential objections to this positioning of translation as central to public engagement and, vice versa, of translation as a broader model for public deliberation and ethical debate. I hereby build on the sense in which the model for translation as superimposition that I have developed throughout this dissertation presents an alternative ethical model for public deliberation on biotechnology.

Charis Thompson offers a detailed analysis of the role that potential 'cures' have played in the history of garnering support for stem-cell research in the United States. She warns that a rhetoric promising cures is misleading when the concerns included in the framing of the public debate are, what she calls, 'pro-curial',[5] where the translation of basic

5 I use the idiom of superimposition throughout this thesis, in part to extend the range of geological imagery

science into clinical applications is closely linked with a search for new marketable innovations. She offers the example of the 'pro-cures' rhetoric used in California's 'Proposition 71', the California Research and Cures Act. In the report, supporters of stem cell research were cast as "fighting to cure pervasive and devastating diseases and medical conditions" (Thompson, 2013, p. 37). By "equating the fundamental ethical imperative to save and improve lives with the provision of state funding for embryonic stem cell research", she writes, "the pro-cures rhetoric counter-balances pro-life objections to research" (ibid.).

Thompson goes on to show how this emphasis on cures played out in the translational climate of the early 2000s:

> To make plausible this ethical claim that the point of the research was cures, the research had to be shown to be concerned with the entire innovation trajectory, all the way from as-yet-undone basic research science to clinically valid treatments. The common use of the expression "bench to bedside" in connection with stem cell research, and the emphasis on "translational" research, did just this. The bench-to-bedside commitment also lent itself to being read as a commitment to funding a new field of innovation, putting California out ahead of the rest of the U.S., and even the world ... Innovation and cures were bundled together as promise and potential, and potential was what these pluripotent cells had in abundance …
>
> Thompson, 2013, pp. 37–38

This leads Thompson to argue that cures-talk not only reinforces unrealistic ideas about the speed with which science can deliver cures and save lives, but also regenerates "the economic life of the State through getting ahead in this sector", and re-commits "to

---

used in scholarship on the debates to evoke a broader, social and political conception of the issues deemed relevant to discussions about biotechnology, beyond individual health. Charis Thompson's 'pro-curial' landscape is one of several 'bioscapes' that each describe the "constitutive technical, political, and ethical trajectories of ideas, bureaucracy, and equipment" included in the framing of a debate, to emphasise how agency is already an "entrenched, capitalised, and material "interest" bundle of humans and non-humans in a given domain" (p. 56). The pro-curial in particular is a "frame or landscape characterised by principled, if contested, procurement parameters, curatorial practices for accounting for acceptably derived research materials and cell lines, and a pro-cures rhetoric of innovation" (p. 44). STS scholar Sheila Jasanoff uses the notion of 'regulatory settlements' to describe how the policy on biotechnology was made in democratic nations between 1975 and 1995 and how these frameworks provided context for future framings of the issues concerned. In both senses, the deeply embedded, social nature of the debate is foreground, a sense that I echo in my model of superimposing 'domains' of knowledge on the way to building legislation.

the epistemology and fruits of science" (p. 45). The problem for Thompson is not only the unrealistic promises of this cures-rhetoric, but also that this framing of the discussion as centred around individual 'health', itself an elusive concept in an economy where this concept seems defined by the 'possibilities' afforded by available commodified remedies, masks other possible framings of the discussion that would hold the potential to challenge the distribution and accessibility of medical provisions that would, in doing so, have the 'social' questions of systemic injustices, rather than individual wellbeing, at its core.

The medical framing in this way contributes to the "biomedicalization of society", in which social problems are posited as curable medical problems, themselves implying a naturalised norm (p. 49). Thompson now introduces her concept of 'good science':

> Good science connotes the conduct of the sciences that have ethics in ways that iteratively develop the science and ethics of their fields together to the mutually entwined and multiple ends of both robust science and technology, and the greater articulation and mitigation of problems of distributive or other injustice.
>
> Thompson, 2013, p.28

This is a call for a re-framing the question at hand, of soliciting new interest, of doing what she calls the 'ethical choreography' of integration, taking seriously criticisms to open new paths of research, and to deliberate carefully on the issues that are broader than just the question of the status of the embryo.

The case study I have presented in this dissertation describes a very different role for clinical applications in the context of the HF&E debates and, in fact, is characterised by some aspects of the 'good science' that Thompson prescribes. In describing the relationship between the biological model and clinical translation, with a focus on McLaren's role, I show that the case developed by the pro-lobby represented more than an alternative 'moral argument' to the anti-research emphasis on the sanctity of life, as has also been argued in much of the literature on the HF&E debates (e.g. Jasanoff, 2004; Mulkay, 1997), but represented a shift towards a biology that formed one component of a layered case based on overlapping concerns, one in which biology became relative to the domains of social concerns, affect, legal provision, ethical arguments, and clinical translation. This model was developed in response to a question that framed the discussion as a pragmatic one of how the embryo could and should be used, and this question was relayed to various stakeholders, post-Warnock Committee, as a question of the possible clinical application of genetic therapy. This broad clinical aim became PGD by incorporating not only new

techniques, actors and fields, but also distinctions based on what was deemed permissible by a broader constituency, such as McLaren's sense that it was genetic manipulation, not selection, that was considered unacceptable by many. In this sense, clinical translation was part of the iterative evolution, stemming from a reframing of the debate as a social question. The model, then, represented more than a 'counter-balance' to a pro-life argument, but an alternative ethical model entirely.

Susan Squier, in *Epigenetic Landscapes* (2017), uncovers, from a feminist science studies perspective, how the 'epigenetic landscape', an image that, as we have seen in the previous chapter, offers a visual metaphor for embryonic development, has been repurposed in various contexts to think through new questions that extend beyond gene–environment interactions of the early embryo. Squier thereby aims to provide a more productive model for "the intersecting complex systems that are now understood to link scientific and cultural practices – or, more precisely, to reveal them as never having been separate or distinct" (p. 7), and so, posits that a broader understanding of the functionality of landscape as a metaphor has implications for the way that science can be done. One particular image, which reveals the "interactions underlying the epigenetic landscape" (p. 185), incorporates multiple scales "of magnitude and time" in a "systemic view" of development (ibid). This model, similarly evident in developmental systems theory (DST), gives us a "better sense of the relations between scales", as we shift "from interactions of molecules inside cells to those between persons … from the short-term dynamics of a population of organisms in a habitat to the slow procession of generations through evolutionary time" (Oyama 2000, cited in Squier, p. 185). Yet this model, Squier argues, raises an ethical problem: "Who, why, and how decides how many variables/elements to include into this new system of 'multiple scales'?" (Irana Aristarkhova, cited in Squier, 2017, p. 186).

One answer, she suggests, is "to say that the choice should reflect (and is responsive to) the requirements of the context within which the choice is being made", but equally important as reflexive inclusion, is the realisation that "these variables and elements are actually always already included" (Squier, 2017, p. 186) and that ethics is therefore "not about right responses to a radically exteriorised other, but about responsibility and accountability for the lively relationalities of becoming, of which we are a part" (Karen Barad, 2012, cited in Squier, 2017, p. 186). The model I am proposing foregrounds both and works from the realisation that connections between domains are drawn on the path to

temporary, imperfect solutions to a specific question.

This analysis challenges the notion, then, expressed by Thompson, that democratic deliberative models that rest "on an implicit ideal of consensus through reason" can provide "no tools for taking persistent dissent into account or for dealing with challenges to the limits of reason in bioethics" (2013, p. 26). By relating various perspectives on the debates, McLaren shows, an understanding of the limits of a particular domain of knowledge is incorporated into a growing model premised on the relations between these partial perspectives. In this debate that took place with the clinical application 'in-the-making', we have a model of a 'good science' that pre-emptively broadens the framing of the relevant issues, while also addressing the problem of communication by providing a specific question, accompanied by the 'relevant' information for a broader constituency to decide on its desirability.

McLaren's problem-solving ethos, as I have described it, resonates with Thompson's ethical model, not as ethics in Aristotelean or Kantian sense, in which there is an "individual action" or "deontological principle at work", and which has dominated American bioethics, but, in reference to entire social systems, prioritises those dimensions of the ethical "that require reference to behaviour, virtue, training, the acceptability of a means or an end, or a greater good or good unto itself" (2013, p. 63). By opening up the process by which this was implemented by a single individual, I hope to have shown how translation can come to define an approach to policy-making in biomedicine that explicitly centralises the negotiation of clinical aims in relation to other domains, in order to expand the frame of the ethical questions at hand. In this model, the translational imperative is an opportunity to re-evaluate what it means to be 'cured' as a society, while alerting the public to the fact that translation takes time, that cures are unpredictable and that they necessitate basic research.

I now move on to a discussion of how this ethical model pertains to the discussions around the possible extension of the 14-day rule in Britain.

Extending the 14-day rule: The meaning of translation

The question has recently been raised among scientists in the field, after over twenty-five years, of whether it is time to extend the 14-day rule. For scientists, there had been no need to contest the rule since no experiment had come close to culturing an embryo anywhere near to the 14-day limit. In 2016, however, two groups in the UK and the US reported

experiments on human embryos that were sustained in culture for 12–13 days after fertilisation (see, Deglincerti et al., 2016; Shabhazi, 2016). The research group in the UK at Cambridge University led by Magdalena Zernicka-Goetz claimed to have developed a method of culturing live human embryos for thirteen days, only stopping their experiment at this point to comply with the 14-day rule. This possibility of culturing embryos up to the legal limit, as well as the potential therapeutic benefits to be gained from rapidly-advancing technologies, such as embryo editing through CRISPR/Cas9, has led policy-makers and scientists in Britain to ask whether it is logical to continue to protect the 'special status' of the embryo up to 14 days (Hurlbut et al., 2017).

Despite the apparent reluctance of some of my interviewees to include their prior policy work in their scientific remit, when asked for their views on extending the 14-day rule, there was a widespread implicit acknowledgement, acceptance and continued support for the Warnock principles carried forward by the pro-lobby and contained in the HF&E Act. This is witnessed in the fact that the scientists I interviewed – both those involved with the pro-research lobby and those who knew McLaren in other capacities – invoked the social contract established by Warnock's framing as well as the need for a convincing clinical case, as carried forward by McLaren. Interviewees such as Jim Smith, Marcus Pembrey and Virginia Bolton (interviews with author, 2018) all agreed that a convincing case for the extension of the rule based on real clinical possibilities, and on extensive basic research, would be required.

This framing of the task as one of building a case, evidently very much a part of how scientists themselves see the task at hand, is distorted by the framing of the discussion itself as one about the possible extension of a rule. In one sense, this framing is testament to the strong and generally positive legacy of the 14-day rule as a policy innovation. Even those who criticise the rule using that common misnomer, 'arbitrary', discuss it as such. David Baird, Emeritus Professor of Reproductive Endocrinology, who also worked at the same MRC Unit as McLaren in Edinburgh, said in an interview with me, "The 14-day rule is a political issue. It is ridiculous to have an arbitrary limit, and it is arbitrary, but it is not ridiculous to start by discussing this rule" (11 September 2018).

Yet I argue, in what follows, that this framing of the question as one of simply picking a point in development performs the same amnesia, the scientific literalism that I noted in scientists' recollections of the debates in Chapter 1, by which the process necessitated in arriving at this line in the 1980s is forgotten. More useful, I will argue,

would be to recapitulate the process itself by exploring clinical possibilities through public deliberation.

Translating lines in biomedicine today

The 14-day rule served explicitly as the point of departure at a two-day meeting that I attended in December 2018, held at the Brocher Foundation in Geneva. At this meeting, titled *Drawing the Line: Assessing & Analysing International Guidelines on Human Embryo research*, the conclusion of the two-day discussion was, especially for the scientists and sociologists in attendance, that perhaps it was no longer a line that we are really looking for at all. This conclusion emerged especially from the scientists' accounts of the relationship between the rule and their practice. Scientists, especially those working on the cutting edge, were often wary of crossing the line in situations that were so different from the research being done in the 1980s, that the 14-day limit no longer seemed to apply. Professor Ali H. Brivanlou, for example, spoke about his work on embryo-like structures at the Rockefeller University. Brivanlou uses *in vitro* attached human embryos and genome-edited 'synthetic embryos' derived from human embryonic stem cells to unveil the molecular, cellular, and embryological basis of early human development. He claimed that his work was testament to the fact that technology has now surpassed the law, that the 14-day rule becomes redundant when you can bypass this limit by creating synthetic embryos by means other than fertilisation. Biologists, now, he said, 'make things work to show they understand'. This makes it hard, he says, to discuss fields only connected by the notion of the 'embryo', or 'pre-embryo', up to 14 days, because these entities may resemble embryos only in part and were created by means other than fertilisation – in short, they challenge the ontological basis of the embryo as an entity in the first place. Like the Warnock Committee, then, it seems that an approach that asks how biological material is being used is more apt than setting rules on how to treat a presupposed coherent entity. Yet this becomes more complicated given the plethora of uses and approaches surrounding embryo-like entities in laboratories today.

The scientists I interviewed for this project, particularly those still practicing, emphasised a similar point about the redundancy of the 14-day rule in light of the kind of work they were doing. Often, they emphasised this as an argument against changing the 14-day rule. Jim Smith, for example, said,

Now that we can go up to 14 days, we are in a position to try mouse model

> experiments in the human and ask, "Are the signals acting at the same time? Are they the same signals? Are they acting in the same way?" You know, there's a lot of basic stuff you can do. And I think let's get that stuff done before we think of going further ... there are opportunities with stem cells for instance in regenerative medicine. Regenerative medicine, stem cells, populating embryos with stem cells – that'll be the way to go.
>
> Smith, interview with author, 23 October 2018

In sketching the research possibilities and questions that can be asked within the 14-day period, Smith points to the research avenues that challenge any sense of temporally linear development. When stem cells can be derived from somatic tissue, when embryonic stem cells are used towards regeneration in adults, the notion of 14 days 'into development' makes fairly little sense. British clinical geneticist, Professor Marcus Pembrey similarly says,

> It's not a good time to change it [the 14-day rule], that's the point. It may be justified in the future. Don't forget that we now can take development backwards, as it were, you start with somatic cells and go back to stem cells…
>
> Pembrey, interview with author, 17 October 2018

Again, Pembrey refers to the possibility of altering the temporality of development to suggest that a change to the 14-day rule is unnecessary and is not the question to be asking given the state of the field. Perhaps then, the legacy of the line that we need to evoke, is not of its literal image, but of the intersections between clinical, scientific, legal, affective and ethical processes that it demarcates.

Our shared biological model demonstrably looks different from the IVF model that formed the point of reference in the HF&E debates, and this is expressed in a range of uses and applications across the private and public sectors globally. Instead of discussing a change to the rule, perhaps we first need to ask, "What research is being done and how and where? What are the research possibilities going forward?"

The time is ripe, participants at the 2018 Brocher meeting suggested, for new metaphors. The favoured image, suggested by Dr Guiseppe Testa, director of the Stem Cell Epigenetics Unit at the European Institute of Oncology, in his presentation to the group, was that of the 'matrix'. It would make more sense, he argued, to speak, when discussing a field filled with embryo-like entities, in terms of their context: as nodes in a matrix of intersecting lines, lines that represent different fields, different research projects, different aims. This consensus seems to be widespread, also evident in the interviews conducted with

a range of members of the embryo research community. In a 2017 interview (Hurlbut et al., 2017), in which most emphasise the need for case-by-case assessment and the articulation of new "ethical principles", rather than new "lines in the sand" (Huyn, in Hurlbut et al., 2017, p. 1032).

Given the complexity of uses and parties involved in research today, the attendees at Brocher concluded the 'line' was an outdated metaphor for discussing a regulatory future. Procedural descriptions would be more useful because these will encompass a broader context for understanding how these biological entities are being used and will thus contribute to a relational definition of their contested, conflicted, multiple meanings that will be more complex but also, crucially, much more representative of their place in society today.

Metaphors and dialogue

Interestingly, though, discussions at the Brocher conference largely skirted around how this re-evaluation of the 14-day rule might involve the public beyond the scientific community. It seems that in returning to the 14-day rule, a key aspect of its meaning was neglected, namely, that it was the outcome of a process of deliberation much broader than the practices within science itself. The line never described the process of deliberation, which was one in which the very relationship between science and society, the divide between experiment and application, was negotiated. We might take note here, of McLaren's version of the matrix, which was one based on the superimposition of science with social, affective, ethical, and legal considerations in order broaden the discussion of the ethics of research practice as a social one.

We also return to the role that clinical application can play in this process of deliberation. What if the matrix of uses of 'embryo-like entities' is instead constructed as a series of possible clinical agendas, broad 'bioscapes', to use Thompson's terms, that can be presented to the public and explored in terms of their social, affective, ethical, and legal implications through public engagement of scientists with the public as they work through the biology necessary to make each of these 'work'. In this way, clinical applications become part of a process of broadening the scope of discussion, rather than reaffirming the status-quo by mapping existing practices under the guise of a 'new metaphor'. In other words, we need a non-literal, sense-for-sense translation of the Warnock-McLaren process of superimposition, which we find in a broader conceptualisation of translation as a multi-

faceted process.

Luckily, as we have seen, scientists are adept to the kind of thinking required to perform the work of using, rather that describing science in order to understand. What Brivanlou is describing in reference to his synthetic embryos, is the epistemological model of synthetic biology also described by Sarah Franklin in *Biological Relatives* (2013a). Embryology now operates according to an ethos in which the embryologist creates in order to understand; where technology always surpasses what is known about biological systems, and where the biological model thereby becomes a fusion: "what is biological, what is a biological mechanism, what is an experimental apparatus, and what is an experimental tool are deliberately rendered opaque" (Franklin, 2013a, p. 134).

These entities are created and might or might not translate into clinical applications, yet this possibility is always very much in the background, marking a shift in practice in which basic science and clinical translation are closely entwined. I have shown using the case of McLaren and the 'pre-embryo' in the HF&E debates, and by reference to her career more broadly, how this ethos of synthetic biology can be usefully applied to policy-making as well as basic science. McLaren pushed for the clinical translation of PGD in order to substantiate a biological model that was more relative, that was assisted, and that came to be widely accepted because it could be demonstrably used towards successful therapies. The clinical application came to stand for a shared goal, expressed in a hybridised biological model that incorporated both the trajectory from embryo to child, as well as the possibility of technological intervention in a continuous biological process. The process of 'making work', then, needs to be seen to incorporate the social work of public deliberation, which needs to be factored into the research agendas that drive science – often, as Franklin (2013a) emphasises, our instruments reach beyond the current state of knowledge, and these instruments include not only the state of the art synthetic embryological models in laboratories, but also the social technologies to which they are inextricably linked.

Here then, is another sense in which clinical applications can provide a conceptual reference point for deliberation on science policy. Like the biological models that are built using tools to explore a set of relations, policy too might model a set of potential relationships, drawing on the propensity of technology to probe beyond what is known, to expand the scope of reference points in discussions about future research horizons. Synthetic biology provides another model for thinking through the policy work that needs to be done today to responsibly regulate in the field of biomedicine in the best interest of

society, by posing the positive, affirmative question of what we are trying to create.

Conclusion

In short, then, this is a timely moment to draw on McLaren's problem-solving ethos. As much as the policy innovation of the 14-day rule itself may no longer be relevant, its underlying logic is still very much applicable today. It is this underlying logic that scientists in Britain and abroad still recognise, although in some cases they seem to have forgotten the implications this held for their role as scientists in the debates. It is also the very logic being used at the cutting edge of embryology, namely the synthetic biology that Brivanlou describes. In order to develop legislation that is attuned to the current state of practices around human fertilisation and embryology, we will need to look at intersections between fields, sectors and domains, in order to broaden our reference points in a properly public debate. The answer to the question, 'How do we make this work?' turns out to be neither strictly scientific nor sociological, ethical, affective or legal, but hybrid – and it comes not in the shape of a line, but as a process that works reflexively on the fractured ground of intersecting concerns.

Conclusion

In this thesis, I set out on the unprecedented task of writing a sociological biography, of telling the story of Dr Anne McLaren's personal, scientific and political formation, in order to map the debates on human embryo research in Britain between 1982 and 1990. Anne McLaren (1927–2007) was a developmental biologist renowned, among other things, for her pioneering work in mammalian embryo development and on the techniques that made IVF in humans possible, and was the key scientist on the Warnock Committee tasked with advising government on legislation following the clinical translation of IVF to humans with the birth of Louise Brown in 1978. McLaren's varied and pervasive role as both a scientist and as member of various committees throughout the debates meant that she provided an indexical view onto the intersections between the legal, social, ethical and scientific questions as they arose. I aimed to bring this role to light through a series of contextual readings of her writing, taken from a range of previously under-researched archives, as well as interviews with former collaborators. The resulting analysis is a portrait of an era in policy-making, drawn as a set of intersecting concerns about the relation between science and society that serves ultimately as an ethical model for science policy-making framed as a translational question.

I will now outline the key conclusions of this dissertation.

Relative biology and the role of the scientist

In Chapters 1 and 2, I offered a description, through close readings of McLaren's own arguments as well as interviews with scientists about the debates, of the precise uses of biological facts by scientists during the debates, leading to an analysis of a dimension, largely under-developed in previous scholarship on the debates, of the role of biological facts and the scientists themselves in this public deliberation, and the specific model for policy-making that this instantiated. In doing so, I have charted the rise of a more relativistic biology in the wake of the clinical translation of IVF. Drawing on the examples of the use of the term 'pre-embryo' used to delineate the entity before the appearance of the primitive streak around 14 days after conception (Chapter 1), and the development of PGD (Chapter 2), I showed how scientists and their interlocutors over the course of the debates moved from discussions of the status of the embryo, to decision-making based on the idea that biology can be used. In this process, a developmental model based on a continuous

biological cycle, in which developmental potential is largely determined by its environmental context, began to displace the teleological model of development as predetermined to produce human babies used by the anti-research lobby. The potential harm that might be done to a developing human by manipulating early life thereby went from presenting a reason for the protection of the early human embryo, to a justification for further life-giving research on them.

This inversion also points to just how closely the biological model and clinical aims were connected in the arguments for research. My analysis shows that the biological case built for the 'pre-embryo' cannot be separated from those for genetic screening or the hopes presented by other future clinical applications. The term became part of an iterative process of rallying support in favour of research and, in doing so, the biological model denoted by the term 'pre-embryo' represented a set of facts that was used to make a case in favour of research that was justified in terms of the good it could do for society. The case of PGD in Chapter 2, in particular, shows how talking about basic science increasingly became a case of encouraging clinical applications as a proof of concept to validate a biological image of contingent development in the public eye. The pro-lobby case, therefore, hinged not on the ontological distinction between 'pre-embryo' and embryo-proper, but on an ethical argument based on a notion of the social good, instantiated by Mary Warnock's framing of the debate in the Warnock Report (1984).

My analysis thus differs from previous interpretations that treat the ontological distinction made using a biological model and arguments for clinical translation as separable factors (e.g. Gunning and English, 1993; Jasanoff, 2005; Mulkay, 1997), or those that see the role of the term only as one of bolstering the argument for research by delineating a suitable experimental subject (e.g. Lee and Morgan, 2001; Crowe, 1990; Spallone, 1987). I show how the pre-embryo factored into a revised ethical model, and a changing understanding of the biological facts as relative, and lasting, as evident from the public response to proposed changes to the Act since 1990, both in terms of widespread support, and in the nature of the concerns raised. Given that I did not have scope to cover the legacy of this changed biological vision in this thesis, I invite future research to examine how public perceptions of biology have evolved since the 1990 Act.

The model as I describe it can be used to deduce wide-ranging implications about the role of the scientist in public debate: the relationship between science, society, law and ethics; the public perception of the biological facts of reproduction and the role of science

in reproduction; and the legacy of the debates in policy discussions about reproduction today. Most importantly, it allowed me to reflect on the relationships between the scientific model used in public debate and the role of clinical translation, which led me to develop the theme of translation, and to cast McLaren and her role in the HF&E debates as a case study in broadening the conventional use of the term in the post-millennial, 'translational' climate of biomedicine, in order to shift the focus of discussions of clinical application to the social infrastructure on which it relies.

I also tied the changing, 'relative' biological model to McLaren's expertise. I show how McLaren combined authoritative scientific understanding with the idea of moral purpose and social good, necessitating a language that could reflect both. Key to this analysis was the concept of superimposition, which I take from McLaren's own descriptions of her practice, as well as the writing of C.H. Waddington (1905-1975), who was a key inspiration to her as a scientist. This metaphor helps to describe McLaren's method of layering social, legal, clinical, emotional and scientific arguments in order to build a consensus based on overlapping concerns. This allowed me in turn to draw together McLaren's particular role and my analysis of the debates more broadly, by showing how she orchestrated the debates through this model of doing science policy. The idiom of superimposition also contributes to my broader conceptualisation of the process of arriving at a clinical translation, showing that this is never a linear process and should be conceived of as a fractured, deliberative settlement. I argue that incongruences always remain in these temporary settlements, and that these should serve as the basis for ongoing re-evaluation of legislation today.

Style of practice

The concept of superimposition also allowed me to connect McLaren's broader formation as a scientist and a Marxist to her role in the HF&E debates in Chapter 3. I identified the same pragmatic approach, centred around a search for analogies of a problem, a non-literal translation, in constant reference to an overarching model. I avoided, in this biographical chapter, essentialising generalisations about McLaren's character that would do little to challenge dominant models for doing science and policy, but tried to root my description in the same understanding of her idiosyncratic approach described in Chapters 1 and 2. This methodological focus was developed largely as a result of my substantive contribution to scholarship, namely the vast original archival and interview material that forms the basis

of this thesis.

My archival material was drawn predominantly from two tranches of McLaren's personal papers newly opened to researchers at the British Library, as well as material taken from the archives at the Royal College of Gynaecologists and Obstetricians, the National Archives at Kew, in addition to my 22 interviews, several unprecedented, with McLaren's former collaborators, including one of the last with Dame Mary Warnock (1924–2018). The breadth and mass of material used for my research led me to bring to light McLaren's very distinctive method, or style of practice, a term used by the historian of science Evelyn Fox Keller (1983) to describe her approach in her biography of geneticist Barbara McClintock, that describes the idiosyncratic approach taken by a scientist to making sense of their findings and includes how they engage with their research object, how they conduct, devise and interpret their experiments, and how they discuss that process. I show how a distinctive style characterises McLaren's entire oeuvre as a scientist, policy-maker, communist and campaigner for social change.

My analysis of McLaren's overarching approach to problem-solving develops my understanding of the ways in which she enacted a concern with social benefit and human problems throughout her life. By looking at McLaren's personal biography, describing her academic and political lineage in relation to thinking about the intersections of 'science' and 'society', I show how McLaren belongs to a legacy of socialist scientists, particularly the embryologist C.H. Waddington, whose practice was premised on an epistemology in which knowledge is gained by drawing analogies that represent the intersections between sub-systems. By describing how McLaren made similar characteristic use of superimposition in a scientific and political context, I show how her approach represents a distinctive ethos of problem solving that is scientific as well as social and political, that is premised on communication across disciplines and domains of knowledge.

I extend this demonstration, vis-à-vis as of yet unarchived material collected from McLaren's daughter, Susan Michie, to a discussion of McLaren's scientific interests in epigenetic inheritance, revealing how this led her to cross the Iron Curtain at the height of the Cold War to exchange ideas with the notorious agrobiologist Trofim Lysenko, and to advocate a fair evaluation of Russian science in the West. In doing so, I distil the social implications and the particular role the model of translation I described in the context of the debates in the 1980s dictates for the scientist, showing how a similar broad, integrative process that relies centrally on communication and feeling is needed to advance science

and the good it can do for society.

This leads me to thematise the role of communication and comparison, which are key to McLaren's approach across her policy and science. Her faculties for communication and comparison allowed her to build her iterative model for gene–environment interactions, as well as common ground in the HF&E debates, and to uphold her commitment to reconciling the domains of policy, society and science for the benefit of society as a whole. The importance of these activities to her practice leads me to challenge literalist paradigms of doing science, that still seem to prevail in scientists' descriptions of their role in public debate today, and to argue that McLaren's model is more attuned to the realities of science as well as policy-making, and is, therefore, more ethical.

My exploration of the role of the scientist in Chapter 3 also led me to analyse how McLaren, an active Marxist and trade union member throughout her life, reconciled her views with those of the Conservative government with which she had to collaborate to move legislation forward in the 1980s. I did this vis-à-vis historian of science Duncan Wilson's analysis of the ideological resonances between Mary Warnock, chair of the Inquiry into Human Fertilisation and Embryology, and Prime Minister Margaret Thatcher, in office for the duration of the debates. This led me to make an original argument about the role that science played in aligning these individuals with diverging political commitments.

I argue that the literal connection between Warnock's and Thatcher's 'anti-expert' commitment to individual choice appears superficial given the fact that there is no evidence that Warnock, always committed to the ideas of social benefit and the common good, shared Thatcher's neoliberalism. I argue that a shared commitment to science, given Thatcher's own scientific training, better explains the collaboration between these individuals. This commonality forms the basis of a causal, substantial relationship between Thatcher, Warnock and McLaren, which, I show, by reference to correspondence between Thatcher and McLaren, contributed to moving the pro-research case forward. This argument adds to the case I make for the role that science and scientific facts can play in an iterative approach to scientific policy-building, based on deliberation and communication through the biological model.

In Chapter 4, I draw on the description of McLaren's problem-solving model developed throughout the dissertation, to evaluate how it might pertain to science policy

questions in the UK today, as well as the question of the extension of the 14-day rule. I use McLaren to direct attention to the process of arriving at the 14-day rule, arguing that the legacy of the rule should be in this model, rather than in the resulting metaphor of a 'line'. In describing the use of the biological model, and of clinical translation with a focus on McLaren's role, I show that the case developed by the pro-research lobby represented more than an alternative 'moral argument' to the anti-research emphasis on the sanctity of life, as has been posited in previous analyses of the debates (e.g. Jasanoff, 2005; Mulkay, 1997), but a veritable shift towards a model in which biological facts were used to make a social argument in favour of research.

This led me to develop my model for a different kind of approach to the ethical discussions on the governance of embryo research, in which the role of science in society is central, thereby contributing to studies that have explored how biotechnology can be drawn upon to posit new models of governance (e.g. Banchoff, 2001; Jasanoff, 2005; Thompson, 2013). 'Relative' biology, I showed, has ethical implications for the scientist's role in public debate, and I drew on this to posit a model for doing what the sociologist Charis Thompson (2013) has called 'good science'. I suggest that a focus on clinical translations as a starting point for developing research agendas, based on the intersecting domains of ethics, law, feeling, and science, makes the work of public engagement central to scientific professionalism.

Methodological parallels

As I have described, my understanding of biology as relative, and the role of the scientist as a communicator and integrator, emerges through a description of McLaren's style of practice and how this is palpable across the entire body of her career. This process of tracing a particular style of practice, through a method based on close readings, across domains, reveals a parallel between my own method and that of McLaren. What I call a 'hermeneutics of practice' links McLaren's style of practice with my own method, which has in common the same assumptions, namely that repeatedly bringing an underlying understanding, or interpretation, to a situation is transformative and that this effect is cumulative. By reading McLaren's texts across sectors, in the context of the HF&E debates, of her scientific work, and of her political writing, I show how her life can be summarised as a pattern that results from her distinctive approach of feeling, communicating, connecting and facilitating in specific contexts. I show how this constituted a distinctive

pro-research argument and how this provided a model for the relations between science and society more broadly. I myself enact this process in order to explicate it. A close reading of individual acts of persuasion that accumulate to build a view of McLaren's style of practice allows me to trace her specific negotiation of the scientific facts in a public debate, to ask how the tensions in these texts reveal modes of thinking and doing that ran counter to dominant notions of the scientific at the time, and to place these in their historical context to ask what these reflect about the changing relationship between science and society. I repeatedly bring this interpretive practice of close reading to the question of McLaren's style of intervention, offering a reading of a life through the lenses of cultural and political worlds, fulfilling the credo to "display the power of disclosure implied in" the author's "discourse beyond the limited horizon of his own existential situation" of the interpretive school of social science (Ricoeur, cited in Rabinow and Sullivan, 1979, p. 100).

This hermeneutic reading, furthermore, foregrounds the sense in which I am describing McLaren's role in terms of her interconnectedness in a web of discourses by showing how McLaren herself performed a similar process of contextualising scientific problems in relation to society. I therefore draw on McLaren as a case study to show how this interpretive method provides a model for both scientific and sociological knowledge.

In terms of feminist cultural analysis, I am entering into the "contest for public knowledge" through a "recontextualization" of discourses by taking McLaren's own language and context as a point of departure. Reading, I demonstrate with this methodology, is "a creative act that translates seemingly incompatible discourses into a cohesive symbolic representation" (van Dyck, 1995, p.60). I am therefore also pushing a feminist cultural textual interpretation practice to draw a picture of a life and to help us understand that life in its deeply embedded context, shifting the terms of discussion around the history of the human fertilisation and embryology debates, while offering a perspective on that life that is intimate, interconnected, and coherent, and that we would not get any other way.

Legacy

I finally make a point about McLaren's legacy as a scientist. Although never marginal to the scientific community by any means, continuing to hold eminent posts at the Royal Institution, The Gurdon Institute and the Medical Research Council throughout her life, McLaren's legacy is tainted by a sense of misunderstanding, which, much like the legacy

of the ‘pre-embryo’, needs to be explained as a failure to adhere to dominant scientific literalist paradigms. My hermeneutics of practice foregrounds how the meaning of McLaren’s life only emerges from a broad, encompassing description of the ways in which she exercised her style across domains and throughout her life. This interpretive reading falls outside of a scientific convention that demands that key discoveries are explained in the language of a single field – as an objective finding.

The ambivalence around McLaren’s science can be explained and connected to the ambivalent legacy of the ‘pre-embryo’, by means of two central maxims that guided McLaren’s practice. The first is, her distinctive definition of her field of study as ‘everything involved in getting the embryo from one generation to the next’, and the second, is her self-fashioning as a facilitator of science rather than conceiver of big ideas or new discoveries – as a ‘midwife’, as Elizabeth Simpson puts it (2001). These two points stand in curious relation to one another. The breadth of her field as the study of life’s regeneration, on the one hand, suggests that McLaren’s field is fundamental to all branches of science. Yet the latter seems to diminish this contribution as somehow lesser, not ground-breaking. The latter resonates with the recollections in many of my interviews.

McLaren is repeatedly credited by her former scientific colleagues for her ability to move easily in any social circle. While in the context of policy-making this is considered a great strength, these same scientists seemed unable to see how this same ability moved McLaren’s science forward. The disregard for McLaren’s legacy as a scientist mirrors the relative amnesia surrounding the use of the biological facts in the debates. Her contributions to uncovering the mechanism of epigenetic effects and developing the method to deduce these, as I argued in Chapter 3, for which the body of her work is one well-developed argument, is discredited as primarily ‘reproductive’ biology – not of ‘Nobel standard’ – by many of her former scientific community. In these accounts, her distinctive and pragmatic approach of crossing between fields and models, as well as applying herself so fully to policy-work, serves to discredit her as a scientist. I show in this dissertation, how the scope of her work depended centrally on her international, interdisciplinary collaborations, which served to move her from one experiment to the next, constituting over time the proper subject of her life’s work as the gross sum of these experiments.

In a sense, McLaren’s legacy highlights some of the lessons ‘not-learned’ from the history of legislating around IVF in Britain – forgotten policy models, as well as forgotten science. I argue that this represents a failure to read an epistemological model based on

interpretation, and the shifting between cumulative analogies, in which meaning is never definitive, and always defined by a particular context facilitated by the relations between scientist, community and tools. McLaren's style of practice depended through-and-through on a broad, integrative view and this is reflected in the policy model she helped invent, in which the biological model is relative to clinical, ethical, legal and affective demands. In this model, the distinction between 'social' and 'scientific' work is much less clear-cut than the statements by some scientists on her role imply.

For McLaren, at least, the social and scientific represented two necessary aspects of solving a problem. The processing required to integrate evidence into her internal model, for McLaren also means doing the social work of communicating, connecting and translating, because society is very much a part of this machine. Indeed, McLaren's commitment to policy work reveals how deeply her conviction that science was a social project ran, and how she saw the work of developing a social infrastructure to support her scientific aims as an essential part of facilitating science for the social good. As I showed in Chapter 4, this point presents an imperative for the scientist to contribute definitions and distinctions to public deliberation on clinical aims.

In this dissertation, I make a methodological, substantive and analytical contribution to the literature on the human embryology and fertilisation debates, and to the feminist cultural analysis of public debates more generally. I hope that McLaren's model, as I have described it, will serve primarily as a reminder of the work that was done by the 1980s pro-research lobby in order to arrive at legislation, and incite scientists today to engage openly in the process of deliberation, acknowledging that 'non-scientific' language is necessarily a part of the process of arriving at scientific applications. The work of fostering an environment that is conducive to research can and should involve the work of finding models to take on board the views of a wider constituency beyond the scientific community; these views should be implemented, practically, in the form of new scientific questions, taking us forward, as we spiral from one generation to the next.

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Robert Shields to Anne McLaren, 16 March 2005. Add MS 89202/4/38, Anne McLaren (1927–2007) Papers, British Library Western Manuscripts.

Interviewees	Position	Date of Interview
Azim Surani	Director of Germline and Epigenomics Research, Gurdon Institute.	22 August 2018
Chris Graham	Professor, Department of Zoology, Oxford University.	27 September 2018
Richard Gardner	Emeritus Professor Embryology and Genetics, University of York.	27 September 2018
David Baird	Emeritus Professor Reproductive Endocrinology, University of Edinburgh.	11 September 2018
David Whittingham	Professor Emeritus Experimental Embryology, University of London.	3 October 2018
Elizabeth Simpson	Emeritus Professor Transplantation Biology, Imperial College London.	21 June 2018
Hilary Rose	Hilary Rose, Emerita Professor of Sociology, Bradford University.	12 September 2018
Jim Smith	Director of Science, Wellcome Trust; Senior Group Leader, Francis Crick Institute.	23 October 2018

James Gowans	Royal Society Research Professor (1962–1977) and Secretary of the Medical Research Council (1977–1987).	18 September 2018
Marcus Pembrey	Emeritus Professor Paediatric Genetics, UCL Great Ormond Street Institute of Child Health.	17 October 2018
Mary Warnock	Former English philosopher of morality, education, and mind.	21 August 2018
Patrick Tam	Deputy Director and Head Embryology Research Unit, Children's Medical Research Institute, University of Sydney.	28 August 2018
Penelope Leach	Psychologist, child-development and parenting.	1 October 2018
Peter Goodfellow	Arthur Balfour, Professor of Genetics, University of Cambridge (1992 – 1996).	21 September 2018
Robert Winston	Professor of Science and Society, Imperial College and Emeritus Professor of Fertility Studies.	26 September 2018

Steven Rose	Emeritus professor of Biology and Neurobiology, Open University and Gresham College, London; author and social commentator.	12 September 2018
Virginia Bolton	Cofounder of the Progress Educational Trust; Consultant Clinical Embryologist, Guy's Hospital.	16 October 2018